

What Salt for the Dragon's Tail?

THE agreement in Moscow between the Soviet and United States governments will go a long way to revive interest in disarmament, seriously dissipated in the past few years by wrangling within the everlasting United Nations Sub-Committee on Disarmament which has become a sporadic fixture at Geneva and by the beneficent diversion which the SALT talks have created. The formal agreement between the Russians and the Americans that the two super-powers shall in future be limited to a hundred nuclear anti-ballistic missiles each is to be welcomed for two reasons—it will ensure that the nuclear balance which has evolved over the past few years will not be undermined by the development of anti-ballistic missile defences accurate enough to give immunity to the offensive fighting forces on both sides and, at the same time, it will take the edge off military spending in the United States and the Soviet Union, thereby helping to moderate what is popularly known as the arms race. But the Moscow agreement goes further by the undertaking, not yet formalized in a treaty, that the two sides will not in future increase the numbers of strategic missiles at present deployed or in the course of being deployed and that neither will convert sites at present occupied by ballistic missiles of intermediate range into launching pads for intercontinental missiles. Although this interim agreement falls short of what the Russians have been looking for in the past few years, especially in its omission of restrictions on the use of long-range aircraft, it is commendable that the American government has seen fit to put its name and reputation to these mutual restrictions when its own position, on paper at least, is numerically inferior to that of the Soviet Union.

What will Congress have to say about the documents, not yet ratified, with which President Nixon will return from Moscow? The most immediate anxiety, of course, is that the advocates of overwhelming military power will complain that the United States is being asked to settle for a more or less permanent inferiority. Within the context of the present nuclear balance, however, this argument is false. What matters is that the strategic striking force of the United States should be capable of inflicting unacceptable damage on the Soviet Union, and there is no reason to suppose that the existing inventory of ballistic missiles deployed by the United States is insufficient to make this gruesome threat a reality. When the Polaris submarines are replaced, as they will be soon, by submarines carrying Trident rockets with a range of more than 6,000 nautical miles, and when the Minuteman rockets have been improved, the threat of unacceptable damage and thus the deterrent power of the American strategic forces will have been still further enhanced. It follows that Congress should not be allowed to make a meal of the apparent imbalance now agreed upon. In the doctrine of deterrence, enough is enough. And although the treaty and the interim agreement worked out in the SALT talks and signed at Moscow do not explicitly forbid the further development of strategic aircraft or

other military innovations, it would plainly be a great waste of energy and money if Congress were now to chase after the rapid development of offensive striking forces still allowed. The truth is that the Moscow agreements represent a great victory for those in the West who have been urging for the past decade that true stability requires a formal acknowledgment by the Soviet Union of the validity of the American concept of mutual deterrence. It would be folly to sour this achievement by too zealous an exploitation of the points at which the new agreements allow one side or the other latitude for further military development.

The implications of the Moscow agreements for the further progress of disarmament are substantial. In the communiqué put out at the end of the Moscow meeting, the Soviet and the United States governments gave their blessing to the scheme for a European security conference—a proposal hatched several years ago by Eastern European governments and still regarded with distaste or at least suspicion by many governments in Western Europe. One obvious stumbling block is that both the British and the French governments still nurse along their supposedly independent deterrent forces, and fear that in a thorough discussion of the problems of European security, these skeletons would be extricated from the cupboards in which they have for several years been buried. But it is also clear that the governments of Western Europe will shy away from a European security conference at least until they have had a chance to work out a framework for a European defence policy—a task on which work cannot seriously begin until the enlarged European Community has learned at least a little of the new political relationships that will then exist. In the circumstances, it will be unfortunate if the European security conference, like the SALT negotiations, serves as an excuse for continuing prevarication in the Geneva sub-committee. There is, after all, no reason why progress should not continue to be made with the extension of the Partial Test-Ban Treaty to cover underground tests of nuclear weapons. Indeed, such a development would help enormously to remove some of the sense of injustice among governments such as that of India at the way in which the Non-Proliferation Treaty has appeared to be a device for perpetuating the nuclear hegemony of the super-powers.

There remains the question of mainland China, already a nuclear power and likely soon to be a force to be reckoned with internationally. None of the agreements about which the Soviet leaders and the American delegation in Moscow congratulated themselves has a single mention of this crucial matter. Yet it cannot be overlooked that the reasons given for the first deployment of anti-ballistic missiles in the United States were largely dependent on the American view of the emergence of Chinese nuclear power. For the time being, at least, the only means of delivering Chinese nuclear weapons are the medium-range bombers being built in China at the



rate of two or three a month on the pattern of the Russian TU-16 aircraft, a few of which were supplied by the Russian government before relations between the two nations were soured in the late 1950s. Ever since the first Chinese earth satellite was launched in April 1970, however, it has been clear that the development of an inter-continental ballistic missile is on the cards. In the past few months, the Chinese appear to have arranged that the missile-testing station at Shuang-ch'eng-tzu should be equipped to fire long-range missiles over a range that crosses India and reaches towards Zanzibar where arrangements are at present being made to monitor the final phases of re-entry. It is anybody's guess how soon these developments will lead to the installation of an effective striking force in mainland China, but there is very little doubt that by the late 1970s, China will be rubbing shoulders with the existing super-powers. Already the existing force of military aircraft should be sufficient for the Chinese to be able to deter the Russians from conventional military excursions to the East. What will happen to the agreements now signed in Moscow when there are enough inter-continental missiles in China to present the United States with a threat to be taken seriously? No doubt the clauses in the new agreements which allow the signatories at Moscow to break the restraints which they have voluntarily accepted if circumstances should change would then be invoked. In short, there is a danger that the agreements will be eroded by the late 1970s unless the super-powers have managed to reach an accommodation with mainland China before the end of this decade. Given the difficulty of negotiating limitations on nuclear striking forces which have become apparent at the SALT talks in the past few years, there is no time to waste. The most urgent corollary of Moscow is that both parties to the agreement should begin at once to persuade the Chinese that sooner or later they will have to follow suit.

Russian Collaboration

PRESIDENT NIXON has brought back from Moscow not merely an agreement on ballistic missiles but a whole sheaf of agreements to collaborate on scientific and technical matters (see page 247). The question now is how these will function. The danger is that the long-prepared and over-dramatic collaboration already worked out for the docking of space vehicles, one from each country, will either unreasonably dominate or will distract from the work that needs to be done in less spectacular fields of scientific research and development. The trouble, of course, is that it is very much easier to sign a joint declaration to do good works than to accomplish them. Over the years, the experience of the working of agreements on collaboration in science and technology between governments in the West and the Soviet government has not been entirely happy. For one thing, they have usually been implemented on too small a scale, at least where the exchange of people is concerned. Not nearly enough attention has been paid to the importance of short visits by people with something of urgent importance to communicate. Too much care has had to be exercised to ensure that the conditions of "mutual benefit, equality and reciprocity" which now crop up in

the agreement between the United States and the Soviet Union are met to the bureaucratic satisfaction of both parties. And although there are welcome signs of willingness to collaborate on actual research, as for example in the exchange of research groups between CERN in Geneva and Serpukhov in Siberia or between the thermonuclear research laboratory at Culham and Professor V. Artsimovitch's group in the Soviet Union, very little has been done so far to plan research programmes which entail a division of labour and an exchange of information extending over several years.

As things are, several obvious opportunities for improvement exist. Thermonuclear research is one obvious field. But here it would be tactful if the Soviet and American governments were to recognize that they are not the only countries with a finger in this pie. Would it not be best if they were somehow to work out a framework for collaboration that could include other interested parties, the official organizations of Western Europe and even the smaller research groups elsewhere in the world, in India and Japan, for example? After all, there is a danger that too close or too exclusive a collaboration between the super-powers may merely exacerbate the sense which other nations enjoy that they are once more unreasonably and unfairly excluded. But is there not the strongest possible case for using the International Atomic Energy Agency at Vienna as a vehicle for enlivening this field for potential collaboration? And much the same is true of basic space research—although the Soviet Union and the United States have more to contribute to a common programme of research than the rest of the world put together, it would be a great mistake if they were now to set up bilateral arrangements for collaboration which give people elsewhere a sense of unreasonable exclusion. The difficulty, of course, is once again that it is harder to ensure that each party to an agreement on collaboration gets his money's worth in the framework of a multilateral agreement than if there is a simple bilateral exchange of information. This, however, should not deter the two governments now relaxing after their exertions in Moscow. After all, the wider the collaboration, the quicker will be the progress they can all expect.

European Medicine

ON the face of things, there should be greater difficulties in bringing about a coherent development of medical practice and research on a European basis than in rationalizing other branches of science and technology, which is why it is at once courageous and imaginative that a group of professors in schools of medicine within the enlarged European Community has published a prospectus which calls for nothing less than the integration of the European medical community. The group has called itself the European Biomedical Research and Education Programme and appears to represent a great many of the European medical schools with a reputation in research as well as teaching. One of its complaints is that the present organization of European medicine and medical research is too much organized on national lines, with the result that medical care is not as good as it might be and that medical research is fragmented and in consequence unreasonably dominated by what happens

in the United States. The group is anxious that there should be a continuing organization to promote cooperation in medical research in the hope that this will lead eventually to a coordination of arrangements for medical education. The plan is now to be presented to the European Commission as well as to governments of member nations. With luck, the outcome could be a valuable European institution for medical research and practice, and a precedent which other disciplines might follow as well.

Although the Treaty of Rome embodies the pious hope that qualified doctors should be allowed to practise where they choose within the European Community, experience so far has been discouraging, to say the best of it. Not merely have the official organizations which register doctors in practice been slow to take a lead from the Treaty of Rome, but there remain wide differences of practice and procedure between the different European countries. And it will be some years before doctors brought up to work within, say, the British National Health Service will be able without the greatest difficulty to find themselves jobs in France or Italy. Not merely do conditions of work and the available facilities vary enormously from one country to another, but patients have different expectations. Yet, as the group of European medical professors points out, the persistence of these differences is a symptom of the complacency and conservatism from which European medicine as a whole now suffers. More mobility of qualified doctors, even at the level of general practice, would help to put an end to chauvinistic smugness. As things are, however, there is very little prospect that the systems of public health now operated by individual governments will be harmonized, as they should be, or even that there will be agreement within the foreseeable future on such mundane matters as the standardization of medical statistics. And opportunities for pooling specialized talents in hospitals and medical schools are at present forgone for lack of what should be the free and easy mobility not merely of medical people but of their patients.

What is to be done? The European Biomedical Research and Education Programme has taken the view that it will be easiest to bring about a full coordination of European medicine by concentrating on research and teaching. Few will doubt the accuracy of this conclusion, but there will be many reservations about the programme's diagnosis of present troubles, at least so far as the American influence on European medicine is concerned. The programme's documents argue that in present circumstances European researchers are dependent for the flow of information or innovation on visits to the United States, on American publications and on the publication of their own original research in the American scientific press. The argument continues that "the overwhelming influence of the vast American research community sets the standards of excellence in science". It goes on to hold that there is for practical purposes "an intellectual dependence" of European research on the "American scientific establishment" with the result that European doctors and researchers think first of buying American and not European equipment, that European biomedical industries are hampered, that insufficient use is made of opportunities for sharing expertise on a European basis and that European biomedical research and education are in some sense emasculated by the

dominance of the American scientific community. To some extent, of course, these complaints are descriptions of the truth. The fallacy in making so much of them is that the programme of development now put forward is in danger of putting the cart before the horse. It is unthinkable that European medical research and practice will be able to stand on equal terms with the American until European governments, separately and collectively, are willing to spend resources on a scale comparable with that now established in the United States.

The specific suggestions for reform now put forward are a modest beginning, but only that. The programme would make an inventory of biomedical research in Europe in the hope of alerting research workers to expertise available on their own doorsteps. It has sensible suggestions for research fellowships, postgraduate and postdoctoral, on a European basis and a scheme for mounting research colloquia on a European basis. More distantly, it wants to see collaboration on the production of textbooks and instructional materials but says nothing, at this stage, about the development of a common European curriculum in medicine. And, of course, there is at this stage, quite understandably, no specific proposal for the removal of the barriers which exist at present to the movement of people within the medical profession from one part of Europe to another. With the best will in the world, that task will be the most difficult of all. Yet, however slow the professional organizations may be, the European Biomedical Research and Education Programme has hit on the only hopeful starting point—the notion that universities and medical schools can on their own initiative do a great deal to set the scene changing.

100 Years Ago



OUR NATIONAL INDUSTRIES

IT is believed by many scientific men that research is all but dead in England. Whether we confess it or not, England, so far as the advancement of knowledge goes, is but a third or fourth-rate power. It is not our present purpose to inquire into the causes of all this; whether, as some say, it is because our professors are so rich, or whether, as others affirm, because all arrangements for the increase of knowledge are so poor, but rather to call attention to the certain influence of this on the wealth—let us put it in the most sordid manner—of the nation in the future.

Surely if scientific men are convinced, first, that the future of our national industries depends upon research, and secondly, that there is no research, the time has arrived when action of some sort is incumbent upon them if they are ever to take action in any subject whatever; for it is perfectly obvious that any bettering of such a state of things can only proceed from the action of the scientific men themselves.

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OLD WORLD

Flowers to Leave the SRC

SIR BRIAN FLOWERS, chairman of the Science Research Council, will succeed Lord Penney as Rector of Imperial College, University of London, on October 1, 1973. The announcement, made at the end of last week, comes only three months after Sir Brian agreed to continue in his present position for a further three years after his term of office expires in September of this year.

Sir Brian has made no secret of the fact that he hoped eventually to return to academic life, but last week's announcement comes as a surprise, particularly after Sir Brian's strong affirmation in February that he was going to stay on "to steer the SRC and the research council system generally through this difficult and confused period".

A spokesman for the Science Research Council said this week, in Sir Brian's absence abroad, that Sir Brian wished to emphasize that his acceptance of the position at Imperial College did not signify a change of mind because no approaches about the rectorship had been made to him when he accepted the offer of a further three years at the helm of the SRC.

The extra year will, however, enable Sir Brian to initiate any changes within the council that prove to be needed in the light of the government's white paper on the future of government research and development, which is due to be published later this month. Discussions in which Sir Brian is expected to play a major part are about to start between the government and the research councils on the government's proposals.

Although Sir Brian's departure from

the SRC will be regretted by the scientific community, which sees Sir Brian as a champion of its cause for retaining the independence of the research councils, there is little doubt that the Department of Education and Science will heave a sigh of relief now that the date of his departure has been announced. It is an open secret that the DES and Sir Brian have not always been in complete accord.

The scene for Sir Brian's move was, in fact, set earlier this year when it was announced that he had resigned the Langworthy chair of physics at the University of Manchester, from which he had taken unpaid leave of absence in 1967 to be chairman of the SRC. This left him open to any other offers from the academic world that might come along.

Lord Penney, who will be 64 in September 1973, has decided to retire although there was no compulsion for him to do so. He has been rector since 1967 and before then he was chairman of the United Kingdom Atomic Energy Authority. Sir Brian has a similar background to Lord Penney, having spent the war years working on the Anglo-Canadian atomic energy project at Montreal and Chalk River and the post-war years involved in theoretical nuclear physics work at the UKAEA establishment at Harwell. In 1958 he took up a position as professor of theoretical physics at Manchester from which he came to the SRC.

Sir Brian's move to a university position in London is in the tradition set by his predecessor, Sir Harry Melville, who, after seeing the SRC through its teething troubles from 1965 to 1967, left to be principal of Queen Mary College.

MEDICAL RESEARCH

Embryonic Protection

THE adoption of a code of practice for the use of foetuses in medical research, and the creation of a central body to advise on ethical matters, are the chief recommendations contained in the report of a government advisory group published last week (*The Use of Foetuses and Fetal Material for Research*, HMSO, £0.185).

The advisory group, convened by the Department of Health and Social Security under the chairmanship of Sir John Peel, the Queen's gynaecologist, was established in May 1970 following accusations that abortion clinics were selling foetuses to medical researchers. The accusation came from Mr Norman St John-Stevan, MP, who was told by an anonymous "medical worker" in a London hospital that foetuses were "being kept alive for up to 40 weeks" for experimental purposes. This accusation proved to be unfounded and indeed to be scientifically impossible given the current state of knowledge, but one clinic had been selling foetuses for research purposes. The advisory group was set up within days of the accusations being made and clinics were warned that if they sold foetuses in future their licences would be revoked. Nonetheless the subject has remained an uneasy one and the advisory group's proposed code, if it is adopted, may well help allay public fears.

The chief points in the suggested code of practice are that because of advances in medical science the present legal definition of a viable foetus (one

that can live as a self-sustaining whole, independently of its mother) should be changed, so that a foetus is considered viable at 20 weeks instead of at 28 as at present; that a foetus which is viable after separation from its mother should not be treated in any way which is not consistent with promoting its chances of survival; and that nothing should be done to a foetus during pregnancy to ascertain what harm it might suffer (for example by the mother taking drugs), even if the mother intends to have an abortion later.

The advisory group, however, defends the use of foetuses and foetal material for research, both when the foetus is born dead and when it is born before it is viable, provided certain conditions are observed. Pre-viable foetuses should be used for research only when they weigh less than 300 g (a foetus of 20 weeks will weigh between 400 and 500 g) and research on dead foetuses should not be restricted provided the relevant laws are observed.

Any suggestion of payment for foetuses, other than necessary costs, is strongly condemned by the advisory group. Research involving pre-viable foetuses should be carried out only in departments directly related to hospitals. Ethical committees should be set up in every institution where such work is undertaken, the committees to be guided by the suggested code of conduct. The group also recommends that a part-time central advisory body should be established with members from the Medical Research Council, the Royal College of Obstetricians and Gynaecologists, the General Medical Council and the British Paediatric Association.



Sir Brian Flowers, Chairman of the Science Research Council.

INDUSTRIAL RELATIONS

APST

THE Association of Professional Scientists and Technologists (APST) is spreading its wings and learning to fly at least as far as the Industrial Relations Court and the Monopolies Commission. Last week the association submitted a statement to the Monopolies Commission on the takeover battle between Boots and Beecham for Glaxo, which the commission is at present investigating.

The association asked the commission to ensure that scientists' jobs were secure in spite of the takeover moves, and argued that if the British pharmaceutical industry is to continue to play a large part in the expanding world market in drugs it is necessary that research of a high standard be carried out. To ensure this, scientists in the industry must hold permanent appointments and there must be an acceptable career structure.

But the APST has not only been making representations to the Monopolies Commission. It has also appeared before the Industrial Relations Court, along with its friend and mentor the United Kingdom Association of Professional Engineers. The association is fighting for recognition as the bargaining agent at both C. A. Parsons, the Newcastle upon Tyne engineering company whose recent past has been anything but peaceful, and at Rolls-Royce (Derby), the marine engineers. The association is also negotiating with two other employers for recognition, but hopes it will not need to have recourse to the courts to obtain this.

The APST was formed in October last year at the instigation of the Council of Science and Technology Institutes (see *Nature*, 233, 440; 1972). But an energetic recruitment drive, intended to produce between five and ten thousand members for APST within a few months, failed to raise the numbers expected. Following a merger with the British Association of Chemists (BAC), however, which voted overwhelmingly to join with APST last month, the membership of the association will stand at more than 3,000 by next month, when the merger should be legally completed. One of the advantages of the merger is that it will allow APST to move out of its temporary offices at the Royal Institute of Chemistry and into the BAC's headquarters in Harley Street. This is a move that Mr Barry Henman, acting secretary of APST, will welcome, for one. He said last week "People have made it very clear that they do not want the association to be the poodle of the institute".

Mr Henman says that he is well pleased with APST's progress although no employers have yet recognized the

association and the huge influx of members that he was hoping for last year failed to materialize. Although the membership is not large the association has five or more members in almost sixty companies. Mr Henman hopes that these members will be the founders of thriving union branches.

The association is also pressing ahead with its plans for a Confederation of Professional and Executive Associations—a professional man's TUC, but working within the Industrial Relations Act (see *Nature*, 234, 117; 1972). That will be an uphill struggle which only a youthful body like the APST would tackle.

Three working parties, with members from APST, UKAPE and the Association of Supervisory and Executive Engineers, are studying the problems involved and trying to write a constitution for the confederation. Progress has been slower than expected—it was originally predicted by UKAPE that the confederation would be fully active by January this year—but Mr Henman hopes to see the new body in action towards the end of the year. "It is," he says, "not the sort of thing that can be dreamed up overnight."

ELECTRICITY

On Tap at Dinorwic?

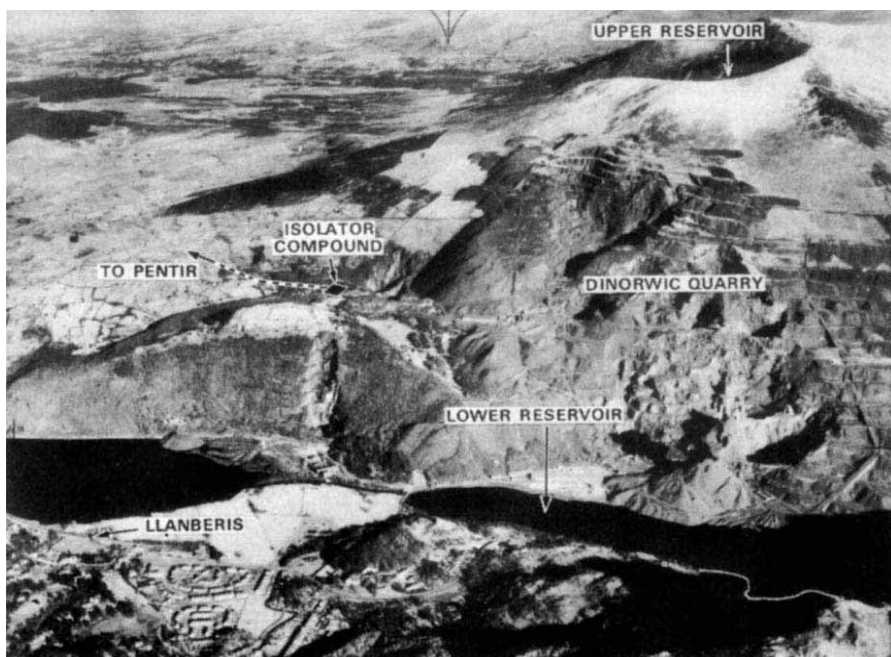
THE Central Electricity Generating Board last week announced its intention, subject to parliamentary approval, to start work on a 1,400 MW hydroelectric pumped storage scheme at Dinorwic in the Snowdonia National Park. If the plans are given the go-ahead, Dinorwic will become the second pumped storage system to be operated

by the CEBG; the first, the 360 MW Ffestiniog power station, is also situated in Snowdonia, and was commissioned in 1963. The cost of the Dinorwic project is estimated to be about £75 million, and the board hopes that it will be completed by 1979 at the latest.

Pumped storage systems work on the simple principle of using spare electricity during periods of low demand to pump water from one reservoir to another higher up; when demand is high, the additional water is allowed to flow through a hydroelectric power station and back into the lower reservoir. The advantage of such an arrangement is that electricity can be fed into the National Grid with very little delay. The Ffestiniog power station can, in fact, be started up and shut down automatically and within seconds by means of frequency sensitive relays.

An aerial view of the Dinorwic area, looking north, is shown in the Figure. The CEBG plans to make use of a lake, Llyn Peris, as the lower reservoir and Marchlyn Mawr reservoir as the upper one. The power station itself, the board says, would be built in a cavern some 175 metres long, excavated underneath the disused Dinorwic quarry.

If it comes to fruition, the scheme will involve the movement of about 6.5 million cubic metres of water between the reservoirs each day; this means the plant could generate 1,440 MW for some 5.4 hours a day and that 6 hours a day would have to be set aside for pumping. A necessary consequence is, of course, that both reservoirs would have to be enlarged to allow fluctuations in the water level. The CEBG envisages an embankment 13 metres high at each end of the lower reservoir and a single embankment about 40 metres high at



The Dinorwic area.

The CEEB spent almost two years making trial borings and surveys on three sites in North Wales—Dinorwic, Bowydd and Croesor—and the decision in favour of Dinorwic was made “having taken into account environmental considerations”. The second choice very definitely seems to be Bowydd, which is partly in Merioneth and partly in Caernarvonshire, for the board says that it “does not foresee circumstances arising that would justify development at Croesor”.

SOVIET SCIENCE

Synthetic Asbestos

from our Soviet Correspondent

THE successful production of synthetic asbestos has been announced by the Institute of Geology and Geophysics of the Siberian Branch of the Academy of Sciences of the USSR. It is claimed that this is the first time that asbestos has been produced synthetically and that the Siberian product exhibits physical and chemical properties considerably “better” than those of asbestos occurring in nature.

Interviewed in *Pravda*, Dmitrii V. Kalinin, head of the Laboratory of Hydrothermal Synthesis of Silicates of the institute, explained that the common form of asbestos, chrysolite asbestos, contains too high a proportion of water of crystallization, so that it is heat-resistant only up to some 500° C. A more stable form, amphibole asbestos, containing far less water of crystallization, does occur naturally, but it is extremely rare, especially in the fluoride form which is especially useful for industrial purposes. Accordingly, Kalinin and his team set out to synthesize asbestos in the amphibole form.

The product they have obtained, bluish-grey lumps growing relatively rapidly (1 cm per week), is, they claim, “incomparably more heat-resistant, strong, stable to the action of salts and acids” than naturally-occurring chrysolite asbestos. Until recently, however, the synthetic product was “not entirely satisfactory” for practical purposes, consisting of short tangled fibres. Now, however, it has been found possible to produce synthetic amphibole asbestos in the form of the long parallel fibres needed for industrial applications.

Rothschild Soeur

DR MIRIAM ROTHSCILD, Lord Rothschild's sister, has a notice on her laboratory door at Oxford which reads “I am not my brother's keeper—but do come in”.

NATURE CONSERVANCY

Ecology in Science

A WARNING that ecology must become a scientific subject, and not a label for alarmism, is issued by Dr Kenneth Mellanby in the triennial report of the Monks Wood Experimental Station, published last week (NERC, £0.60).

Dr Mellanby, director of the station, says that one of the unfortunate effects of the growing recognition of the importance of ecology is that “many so-called ecologists, with no real expertise or experience, are trying to jump on the environmental bandwagon. They know that publicity will be given to those who make extreme statements, generally of impending disaster, and that those who prefer to rely on scientific observations will enjoy less popularity. It is essential . . . that ecology becomes a recognized scientific discipline”. To ensure this, Dr Mellanby says, research at Monks Wood, one of the eight British stations of the Nature Conservancy, which is itself part of the Natural Environment Research Council, must be of the highest standards. “Only then will our results be recognized as generally significant.”

In the introduction to the report Dr Mellanby also gives his views on the Rothschild report, although without actually mentioning that dread name. “I agree,” Dr Mellanby says, “with those who say that there are only two types of research—good and bad.” The distinction between research and development when applied to the conservation work and pollution studies that the station undertakes is unreal, Dr Mellanby says. A good worker he continues, may make fundamental discoveries, while working on strictly applied research, simply by realizing the significance of the observations he makes. A poor worker will simply accumulate trivial data.

Dr Mellanby also makes a plea for more popularization of scientific knowledge. “I believe it is the duty of all scientists, but particularly of those whose work is supported by public funds, to try to tell the public what they are doing,” he says. To this end, the staff at the station have written many newspaper articles and appeared in well over 100 radio and television programmes. Also, technical publications run to well over 300 papers in the 3 years the report covers (1969–71).

Monks Wood, with its staff of 110, is Britain's largest ecology and conservation centre and the immensely detailed report gives a lengthy summary of the research projects on which the station is currently working. These include studies on lead pollution near road verges, which have shown levels significantly higher than background

close to the roadside, fading away to background levels within 8 to 16 metres. Work on herons and golden eagles has shown that reductions in dieldrin sales in 1966 have resulted in greater breeding success for the eagles and falls in the dieldrin content of heron eggshells.

ENVIRONMENT

The Polluter will pay

THE principle that the polluter must pay to clean up the environment has finally been accepted by the OECD. At a meeting in Paris last week, the 23 ministers of the organization agreed on the “polluter pays” principle to help allocate costs of pollution control, to encourage the rational use of scarce resources, and to avoid distortions in international trade.

The effect of companies having to pay to clean up their pollution will be to raise the price of goods whose production would otherwise greatly damage the environment. The ministers also resolved that subsidies should not be provided to offset this effect.

The ministers also agreed that more stringent anti-pollution controls are needed, but that care must be taken not to create barriers to trade. Where such controls will affect products that are traded internationally, governments should seek common standards.

These measures are not intended to undermine any country's independence in deciding its environmental standards. In the guiding principles that accompany the agreement, the ministers state that national pollution policies are bound to differ because of different social priorities and different levels of industrialization. In spite of this, attempts to harmonize standards should be made.

The ministers also agreed that in conformity with the General Agreement on Tariffs and Trade, measures taken should be applied to all products sold in a country, whether they are produced at home or abroad.

The Confederation of British Industry was unable to comment on the agreement this week as the clauses were still being studied by the CBI. But it is clear from recent remarks by Mr Arthur Biggs, the CBI's chief technical adviser (see *Nature* 237, 126; 1972), that the agreement should gladden the heart of industrialists, provided that it is applied across the board. Such a general increase on costs, which can then be passed on to the consumer, is industry's preferred recipe for combating pollution. In contrast, the United States has opted for a pollution tax to encourage companies to protect the environment.

NEW WORLD

Scientific Accord in Moscow

by our Washington Correspondent

COOPERATION between scientists in the United States and their counterparts in the USSR has always been *ad hoc*, informal and restricted. But that may be changed by the four agreements on scientific cooperation signed by President Nixon in Moscow last week. According to Administration officials, the agreements, which relate to general scientific and technological matters, health research, environmental problems and space science and technology, will usher in a new era of formal cooperation between the United States and the USSR on all kinds of technological ventures and scientific projects financed jointly by the two governments.

Although the actual signing of the treaties was partly theatre, since all the details had been worked out well before President Nixon set foot in Moscow, their significance may be more than symbolic, for they set up a mechanism by which future cooperative ventures can be planned. But the realisation of the Administration's optimism will depend on whether the various committees and commissions established by the agreements can fashion from the vague language of the treaties a list of programmes and exchanges that will be mutually beneficial to the two governments.

In any case, the only tangible sign of the new agreements during the next few months is likely to be the planning and preparation for the joint docking experiment between Apollo and Soyuz spacecraft that was announced last week.

This calls for the production of a docking module to provide a link between the two spacecraft and to act as an airlock for adjusting the different atmospheres in the Apollo and Soyuz spacecraft. The atmosphere in Apollo is pure oxygen at about 5 pounds per square inch pressure, while the atmosphere in Soyuz is a nitrogen-oxygen mixture at about 15 pounds a square inch. The module will be developed in the US and it will be carried aboard Apollo for the proving flight in 1975. According to James T. Fletcher, Administrator of NASA, if that experiment is successful, it will pave the way for many more joint space projects in the 1980s.

The most far reaching if least tangible of the four agreements is the treaty on general scientific and technological matters. A declaration of intent rather than a blueprint for cooperation, the treaty brings up to date and

greatly expands previous agreements on scientific exchanges between the US and the USSR. Essentially the agreement acknowledges that mutual benefit may be derived from scientific cooperation in science and technology, and sets up a joint commission to plan and oversee such activities.

The joint commission will consist of equal numbers of representatives from the US and the USSR, and will meet at least once a year, alternately in Washington and Moscow. The agencies responsible for servicing the commission and implementing its recommendations will be the Office of Science and Technology in the United States and the State Committee of the USSR Council of Ministers for Science and Technology. Dr Edward E. David, Jun., the President's Science Adviser and Director of the Office of Science and Technology, said last week that he hopes to meet his counterpart in the USSR, Academician V. A. Kirillin, in the next few weeks to plan the guidelines for the commission. If all goes according to plan, the joint commission should meet for the first time in the late summer.

The type of venture envisaged for cooperation between the two governments clearly goes much further than the mere exchange of information or the informal contact between scientists at international meetings that have been the chief means of cooperation in the past. The agreement involves the joint development and implementation of basic and applied science projects, joint research on specific problems, including the sharing of information and research results, organization of conferences and symposia, exchange of personnel and technical information, and the establishment of contacts between US firms and enterprises in the USSR where there is mutual interest. The chief criterion for such an exchange will be simply the possibility of mutual benefit, or, as Dr David said last week, "we want it to be a give and take relationship, not a take and take relationship".

Although Administration officials emphasized last week that no detailed projects have yet been worked out, and that candidates for such joint ventures are many, there are some areas that are now ripe for cooperation. One is high energy physics, a field in which there has already been considerable international collaboration. A team of high energy physicists from California, for example, has already conducted an experiment at the Serphukov accelerator in the USSR, and a team of Russian physicists is now working at the

National Accelerator Laboratory in Illinois. In such a situation, where expenditure on capital equipment is very large, potential for collaboration is clearly great.

Similarly, much mutual benefit could be derived from cooperation on research on controlled nuclear fusion techniques. Scientists in the USSR, for example, have an acknowledged lead in research on the technology of tokamaks for containing hot plasmas, while recent research on laser heating of solidified deuterium, in the United States, has shown some promise. Systems science was also mentioned by Dr David last week as an area under consideration for cooperation.

Although plans for cooperation will evidently be far reaching, Dr David said last week that there is little likelihood of direct investment by the US in research in the USSR, or the reverse. The idea will be that each government will finance its own portion of any research that may result from joint planning of a scientific venture, and the chief benefit, apart from the intangible dividend of improved relationships between the US and the USSR, will be prevention of some duplication of effort.

Details of the actual projects to be undertaken through the agreements on health research and environmental matters have also not yet been worked out, but again, the prospects are good provided the committees established by the treaties can suggest worthwhile and acceptable proposals. Both agreements call for joint planning and oversight committees to be set up to implement the agreements, and the responsible agencies in the US will be the Department of Health, Education and Welfare and the Council on Environmental Quality.

The health agreement simply reaffirms an agreement reached in February between the Department of HEW and the Ministry of Health of the USSR, and elevates that agreement from an accord between the two ministries to a full blown treaty between the two governments. Among the areas likely to be involved in collaborative projects are heart research, cancer and health problems associated with the environment, and the level of activity should, in theory, be the same as specified by the general agreement on scientific and technical cooperation. According to a statement put out by HEW, preliminary discussions have already taken place, and agreement may also be reached on cooperation on problems of the delivery of health services, influenza, occupational health and the organic basis of schizophrenia.

The agreement on cooperation on

environmental problems has been billed by Mr Russell Train, chairman of the Council on Environmental Quality, as the "first comprehensive environmental agreement between major world powers", and although Mr Train pointed out that the discussions that led to the agreement were conducted independent of talks on the UN Conference, they augur well for the prospects in Stockholm. Eleven areas have been specified for long-term collaboration between the two countries, including air and water pollution, pollution from agricultural activities, climate modification and earthquake prediction. Mr Train emphasized last week that the treaty does not reaffirm any existing programme, but that it is "a whole new ball game with the Soviet Union".

The agreement will probably include joint economic studies of new technologies and pollution control measures, which should be an interesting exercise since the two economies are so different, but it is likely first to provide joint cooperation in areas likely to be of short-term benefit to the two countries. One such candidate is cooperation on studies of climate modification associated with environmental changes. US scientists have already built up a substantial body of data relating to the effects of cities on climate, while the thrust of studies in the USSR has been concerned with the climatic effects of water resource projects. The two are clearly complementary. US scientists and engineers are also anxious to discuss with their counterparts in the USSR the problems of building pipelines in Arctic regions. Russian engineers should have much experience in this area which would come in useful for construction of the trans-Alaska pipeline.

The space agreement provides for a joint mission rather different from that being talked about up to a month ago. The intention then was to dock an Apollo spacecraft with the Salyut space station, but in April officials of the USSR changed the plans because of technical difficulties involved in the project. The chief problem was to provide docking facilities on Salyut for both the Apollo and Soyuz spacecraft without interfering with the stability and guidance of the space station, and it was agreed to prove the docking system first with the two spacecraft. The change of plan will, however, greatly decrease the extent of the work that can be carried out on the mission, since both spacecraft are relatively small, and it will probably require that they each be flown by a two rather than a three man crew.

For the flagging US space programme, the agreement is a shot in the arm which should help to maintain

public interest between the last Apollo flight next year and the first flight of the space shuttle in the late 1970s. But it will also help NASA keep the Apollo team intact until the shuttle is developed and will provide a few more jobs for the ailing aerospace industry. Although the projected employment on the project of 4,400 will hardly make a dent in the unemployment problem, however, it is a useful figure to be thrown around in election year. The project is estimated to cost about \$250 million.

ADVISORY COMMITTEES

Senility and Relevance

by our Washington Correspondent

WINSTON CHURCHILL once said of advisory committees: "We are overrun by them, like the Australians were by the rabbits", and he was also heard to remark about Niels Bohr, who had then just been attempting to convince him of the dangers of pressing ahead with the construction of the atomic bomb, "What's he talking about, science or politics?". If he were alive and in Washington now, Churchill would be even more confused about the position of science in the political process, and more irritated by the proliferation of advisory committees, for there are an estimated 2,400 committees advising the federal government, some 1,500 of them concerned with scientific and technological matters.

How well is the scientific advisory system working? In particular, is the best use being made of the available scientific talent? According to a committee of the National Academy of Sciences (NAS), under the chairmanship of Dr Detlev W. Bronk, an elder statesman of the scientific establishment, although the advisory committee system has functioned well so far, the committees have become top heavy with old men while younger scientists, women and ethnic minorities scarcely get a look in. The NAS committee also suggests that many advisory committees have outgrown their usefulness and should be scrapped, and that the mechanisms by which committees are set up and members appointed is in need of overhaul. (*The Science Committee*, available from the Printing and Publishing Office, NAS, 2101 Constitution Avenue, Washington DC 20418.)

A study of the committees of the National Research Council shows that the median age of the members is 50 years, compared with the median age of all US scientists with a PhD of 40 years, and, while women constitute 7 per cent of all doctorate scientists, they make up only about 1 per cent of NRC committee members. (The median age of the committee that produced the re-

port was 59 years, and it contained no women.) Moreover, only about 2 per cent of all NRC committee members come from ethnic minority groups. To help redress the balance, the NAS committee suggests that not only should agencies such as the NRC cast the net more widely in choosing committee members, but agencies which sponsor the studies should include consideration of social and economic questions in the tasks assigned to committees. The NAS committee believes that younger scientists are more willing to consider such questions—a suggestion which would indeed confirm Churchill in his belief that scientists should stick to hard science.

Another aspect of the advisory system that bothers the NAS committee is that problems may be referred to a committee simply to delay making a decision, and that "sometimes an existing committee is formed out of habit or inertia simply because the advisory framework exists and is convenient, without a clear decision that reference to a committee is the best course in the circumstances". Moreover, committees often get stale and their approach to problems becomes a conditioned response rather than a fresh appraisal, and sometimes committees will continue to meet even when their job has been completed.

To combat these ills, the NAS committee suggests that advisory committees should be appointed only when there is a clearly defined objective for them to aim at, and that the rationale underlying their operation should be carefully examined before members are appointed. Both the agency which sponsors a study and the agency which appoints the committee should have an annual spring-clean, reviewing the status of each committee and scrapping those which are no longer needed. Those that pass the senility test should have their members rotated to ensure a constant influx of new ideas.

Changes in the structure and operation of the National Research Council now being implemented (see *Nature*, 237, 6; 1972), may help to meet some of the criticisms outlined in the report. The suggestion to set up registers of scientists willing to serve on NRC committees, and to service some of the more important committees with high-powered staff members on an *ad hoc* basis, for example, may help to keep the committees from getting set in their ways and ideas. But the reverence for experience and the "buddy system" by which committee appointments are made will undoubtedly ensure that the advisory system will be slow to change. The best to hope for is that the appearance of the NAS report will put many to death.

NEWS AND VIEWS

Interacting Radio Galaxies or Radio Trails?

MOST sources listed by radio astronomers are thought to be extragalactic. Of the brighter ones which have been studied in detail, about one third are associated with galaxies, one third with blue stellar objects (quasars) and one third cannot be identified with any optical objects, probably because these last are galaxies beyond the present reach of the largest optical telescopes.

High resolution observations with aperture synthesis telescopes have revealed a wide diversity in the radio structures of these sources. The most powerful, with radio luminosities more than a million times that of our own galaxy, are double and consist of two emitting regions which may be separated by distances as small as 30 and as great as 3×10^5 light years on either side of the related optical object. The most compact sources, which can only be studied by transcontinental baseline interferometers, are often associated with quasars, but the most extensive may, like Cygnus A, be associated with a galaxy, although in other wide doubles the central object is a quasar. There seems to be no way of distinguishing between quasars and radio galaxies by radio means and indeed it is becoming doubtful whether any such distinction should be made. In the case of several extensive double sources, even though no central star-like image can be seen optically, a compact central radio component is found.

One of the fundamental problems is the origin of the vast quantities of energy necessary in such sources (up to 10^{38} – 10^{44} J). In view of the frequent occurrence of compact radio and optical features at the centre of extended double sources, it seems likely that energy is released more or less continuously throughout the lifetime of these sources which must be at least 10^6 years. Gravitational energy seems to provide the only satisfactory supply, and many hypotheses have been made concerning the way in which it is made available; these vary from known processes such as supernova explosions which result in the formation of neutron stars to exotic mechanisms involving collapsed galactic nuclei.

The other problem is how this energy reaches the main radio emitting regions and what prevents the rapid expansion of the components. The energy may be transmitted from the nucleus in the form of (a) relativistic particles, (b) a fast moving plasma or (c) low frequency electromagnetic waves (0.1–1 kHz) associated with the magnetic dipole radiation of neutron stars. All aspects of these problems seem to require the existence of an intergalactic gas.

Radio sources of intermediate power (about 10^8 times that of normal galaxies) often have a more complex structure than the most powerful ones. In 1968 a particularly interesting class of such objects was recognized from studies with the one mile radio telescope at Cambridge in which two galaxies in the Perseus cluster (NGC 1265 and IC 310) were found to have an associated source in the form of an extensive tail in a direction away from that of the active Seyfert galaxy (NGC 1275) near the centre of the cluster. Subsequent observations showed a similar source in the Coma cluster and one in an unnamed cluster (the radio source 3C 129). The radio

tails are of very great extent, that associated with 3C 129 probably being more than 6×10^5 light years in length. It is difficult to understand the very marked curvature of these tails without supposing that there is a significant amount of gas and magnetic field within the cluster. Further evidence for the presence of such gas comes from a study of the physical sizes of double sources inside and outside clusters. De Young has recently shown that those in clusters are of smaller extent suggesting that the gas density may be greater inside clusters than in the general intergalactic medium.

In all known cases these sources are close to a more compact source of comparable radio luminosity associated with an active galaxy in the same cluster. These observations suggested that the companion galaxies were being excited in some way by the emission of relativistic particles, plasma or possibly low frequency electromagnetic radiation originating in the active galaxy. Another possibility is that the motion of the galaxy at high velocity through the intergalactic gas may itself be sufficient to convert kinetic energy into the relativistic particles responsible for the radio emission.

Miley, Perola, van der Kruit and van der Laan on page 269 of this issue of *Nature* present observations of some of these sources with the Westerbork synthesis radio telescope. The authors suggest as an alternative explanation that each galaxy itself contains a source of energy and ejects clouds of plasma containing relativistic particles as in the case of powerful double sources. It is then proposed that if the galaxies have a high velocity through the intergalactic medium, as is observed in the case of NGC 1265, the ejected clouds are stopped, leaving trails of emitting material behind the galaxy. This mechanism avoids the necessity for any association with the other active galaxy in each cluster.

Whatever the ultimate explanation for this strange class of object, there seems little doubt of its importance. First, these sources provide a way of investigating the intergalactic gas in the cluster. Second, the mechanisms involved in containing particles within a relatively narrow tail of very great linear extent are clearly closely related to those occurring in powerful radio galaxies. Third, an understanding of the problems of energy transport and supply in these complex sources should illuminate the much more severe problems associated with the most powerful sources. Fourth, it may be that these objects provide evidence for the conversion of kinetic energy into relativistic particle energy, a process which has often been the subject of conjecture in the past. Fifth, these studies are related to the general question of the dynamical stability of clusters of galaxies.

These sources are thus related to a very wide range of astrophysical problems. From the radio astronomical end of the spectrum, further observations at Cambridge have provided maps of 6" arc resolution at 5 GHz and similar observations are planned with the Westerbork instrument. The new data from these instruments and from the new Cambridge 5 km radio telescope, which should shortly provide a resolution of $\sim 2''$ arc, are likely to prove of great importance.—From a Correspondent.

More Early Hominids from East Rudolf

PALAEoANTHROPOLOGISTS traditionally have had to be content with the study and description of a very limited number of specimens, but because of the recent discovery of important early man sites in east Africa this situation is rapidly changing. Since intensive field work began in 1968 in the East Rudolf area in Kenya more than forty-five hominid specimens have been found by Richard Leakey and his colleagues at the two main sites of Ileret and Koobi Fora.

A tenet, which is basic to many theories of human evolution, is that no more than one type of hominid has existed at any one time. This view has often been challenged but the fossil evidence has been equivocal. Almost from the beginning of the field work at East Rudolf, however, it was apparent that more than one type of early hominid had lived there, and that the two could have been contemporaneous. Richard Leakey's report of the 1971 collection from the area (see page 264 of this issue of *Nature*) further strengthens the evidence for the coexistence of at least two types of hominid during the early Pleistocene in east Africa.

The hominid collection from the 1971 season again demonstrates, as it did in 1970, the existence of the genus *Australopithecus* at East Rudolf; members of this group are, of course, known from Olduvai Gorge in Tanzania and from other sites in east and southern Africa. It is recognized that this genus contains a wide range of variation and the taxonomic significance of this variation is an important question in human palaeo-anthropology today.

One of the most interesting specimens from the last season is the massive mandibular fragment, KNM-ER 818. This jaw demonstrates the now familiar pattern of the large australopithecines—large molars and premolars and reduced anterior dentition combined in a robustly constructed mandible.

The new material also demonstrates, again as it did in 1970, the existence of a more advanced hominid; this material is possibly referable to the genus *Homo*. The cranial vault material is still incompletely known so that a generic diagnosis must, for the present, rest largely with jaws and teeth. Teeth,

however, are notorious masqueraders: one fossil "primate" turned out to be a pig and several isolated "hominid" teeth remain in taxonomic limbo.

Jaws are usually a different matter, although Piltdown's orang-utan jaw fooled more people than one cares to remember. The morphology of the jaw in hominids reflects many diverse but highly interrelated evolutionary trends and the total morphological pattern of fossil mandibular material is usually of considerable taxonomic value, at least at the generic level. The size relationships between the teeth and the general mandibular structure are particularly relevant. With this in mind, it is very difficult to regard the KNM-ER 730 jaw from the 1970 season, and the new juvenile mandible, KNM-ER 820, as anything other than *Homo*.

Much post-cranial material, chiefly of the lower limb, has also been recovered at East Rudolf. Six femora are now known from the area, and although some of this material appears quite modern in many respects, other specimens show a morphology more similar to known australopithecine femora from Olduvai Gorge and South Africa. Of particular interest is the partial skeleton, KNM-ER 803, from Ileret. This is the most complete assemblage of associated hominid material from

the early Pleistocene and a functional analysis of the lower limb of this specimen will be of considerable interest and importance.

Richard Leakey has very wisely refrained from allocating this material to any particular species; in this he shows welcome, if almost unprecedented, restraint in a field cluttered with arbitrary and invalid nomina. His suggestion that some material presently attributed to *A. africanus* should be included within *Homo* is interesting. It has been obvious for some time that existing theories of Linnean taxonomy are somewhat inadequate for dealing with the early Pleistocene hominids—indeed, for dealing with palaeotaxa in general—and a comprehensive revision of this problem is long overdue.

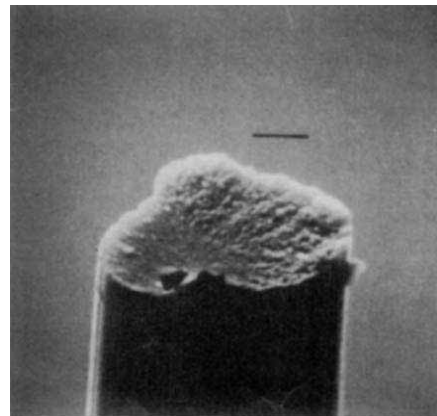
At present, the dating of the Ileret and Koobi Fora sequences must be regarded as somewhat uncertain. No firm dates have been published for the Ileret sites—the suggested date for the Upper Tuff of 1.0 to 1.5 m.y. is based on faunal remains. Although the Pleistocene faunal assemblages are well known in east Africa and are therefore fairly reliable for dating purposes, the possibility of sampling error always imposes an element of doubt for dates so derived. One radiometric date does exist for Koobi Fora—an age of 2.6 m.y. based on potassium/

Carbon Fibres and the Griffith Equation

IN the next issue of *Nature Physical Science* (June 5), W. Whitney and R. M. Kimmel show that the Griffith equation, which connects the total energy required to break a brittle material with such parameters as the breaking stress and strain, the apparent surface energy and the size of the critical flaw at which fracture commences, is applicable to the filaments that go to make up carbon fibres.

Whitney and Kimmel tested several groups of filaments to fracture—a typical fracture surface of a filament is shown in the diagram where the length of the bar is 2 μm . The total energy required and the size of the critical flaw were measured and fitted to a logarithmic form of the Griffith equation. The values of the surface energy which emerged turned out to be quite comparable to the surface energy calculated from the description in the literature of the two fractured ends of another carbon fibre. A certain amount

of variation in the surface energy is, of course, to be expected because the filaments are not ideally brittle materials. If they were, the surface energy would be just that needed to break the chemical bonds, but in real materials this energy is enhanced because plastic work always has to be done before fracture.



argon ratios for a tuff in the Lower Unit. This dated tuff, however, is overlain by an unconformity of unknown duration and this means that the age of the Upper Unit is not known with certainty. The suggestion that the dated tuff at Koobi Fora is contemporary with a similar tuff at Illet, approximately 30 miles away, must also be viewed with caution.

The complex and fascinating picture of hominid history which is emerging at East Rudolf poses a variety of questions. Although none of these questions is really new, perhaps enough material is now becoming available so that answers may be proposed with more assurance than before. — From our Palaeoanthropology Correspondent.

HEPATITIS

Model in Monkeys

from our Medical Virology Correspondent

THE variety of animals which have been used to find a suitable experimental model for viral hepatitis include hamsters, horses, African gerboas, Mongolian gerbils, the egg of the domestic fowl, canaries, pigeons and non-human primates (J. W. Mosley, *Arch. Ges. Virusforsch.*, **22**, 252; 1967). F. Deinhardt and his associates, and others, have demonstrated that species of marmosets, *Saguinus nigricollis*, *S. fuscicollis* and *S. mystax*, can serve as animal models for human type A (infectious or epidemic) hepatitis (see *Nature New Biology*, **229**, 130; 1971), but in the absence of serological or other markers for the elusive virus of infectious hepatitis, this particular monkey model, although undoubtedly useful, cannot be used generally and the test system is still inadequate.

The discovery of the relationship between Australia antigen and type B (serum) hepatitis has now provided the necessary marker for the study of this form of viral hepatitis (*Bull. Wld Hlth Org.*, **42**, 957; 1970) and advances in this field have recently been rapid. William T. London and his colleagues from the National Institutes of Health, Bethesda (*J. Infect. Dis.*, **125**, 382; 1972), now report the successful serial transmission in rhesus monkeys (*Macaca mulatta*) of an agent associated with hepatitis B antigen (previously referred to as Australia antigen). The infection in these monkeys was inapparent and it was not associated with biochemical evidence of liver disease nor with obvious histological changes in the liver. Hepatitis B antigen was demonstrated transiently and antibody responses were recorded in

the animals inoculated with serum from a human antigen carrier with chronic hepatitis at the primary passage. The antibody responses in infected rhesus monkeys were similar in pattern to those observed in man following either natural or artificial infection with hepatitis B virus. Successful transmission was achieved through five serial passages in rhesus monkeys. Vervets (*Cercopithecus aethiops*), on the other hand, appeared to be refractory to this infection.

The finding of hepatitis B antigen and antibody in several nonhuman primates has been reported previously and the immunization with this antigen of chimpanzees (E. A. Lichter, *Nature*, **224**, 810; 1969) and other species had also been recorded; but William T. London and his colleagues rightly believe that their observations constitute the first unequivocal demonstration of serial transmission of type B hepatitis agent in a readily available laboratory animal.

Current progress with hepatitis is therefore gratifying and the availability of suitable animal models for human hepatitis should permit the study of some of the biological characteristics and the physical and biochemical properties of these infective agents, without recourse to experimentation in human volunteers. It is perhaps also timely to recall that all nonhuman primates (as well as other animals) have an indigenous viral flora of their own and monkeys and apes must not and cannot be considered simply as "test-tubes". Kalter and Heberling (*Bact.*

Rev., **35**, 310; 1971) have recently reviewed this problem in a comprehensive report on the comparative virology of primates.

There is a striking resemblance between events in hepatitis and the historical developments which culminated in the conquest of poliomyelitis. At the beginning of the century, Landsteiner and Popper (*Z. Immunitätsforsch. Exp. Ther.*, **2**, 377; 1909) described the susceptibility to poliomyelitis virus of rhesus monkey (*Macaca mulatta*) and baboon (*Papio hamadryas*), and later other monkey species similarly were found to be susceptible to the virus. The discovery by Enders and his colleagues (*Science*, **109**, 85; 1949; and *Amer. J. Med. Sci.*, **220**, 316; 1950) that tissue culture cells of primates including monkey kidney were susceptible to infection with poliovirus precipitated the large scale use of nonhuman primates in the laboratory. These observations provided the technology which led to the isolation of numerous viruses from man and animals and, ultimately, effective methods of prevention of infection were introduced.

Recent reports also offer hope for the development of tissue culture systems, which may also be suitable for the propagation *in vitro* of hepatitis B virus (A. B. Jenson *et al.*, *Exper. Mol. Path.*, **13**, 217; 1970; D. H. Carver and D. S. Y. Seto, *Science*, **172**, 1265; 1971; and A. J. Zuckerman *et al.*, *Nature*, **236**, 78; 1972). The prospects for active immunization of man against type B hepatitis thus seem brighter.

Two Kinds of Pulsarquake?

THE sudden changes in period which occur in the pulsars with shorter periods and are known as spinups or glitches seem to fall into two categories, according to D. Pines, M. Ruderman and J. Shaham. Writing in *Nature Physical Science* (next Monday, June 5) Pines *et al.* point to differences in the behaviour of the Vela and Crab pulsars at the time of glitches.

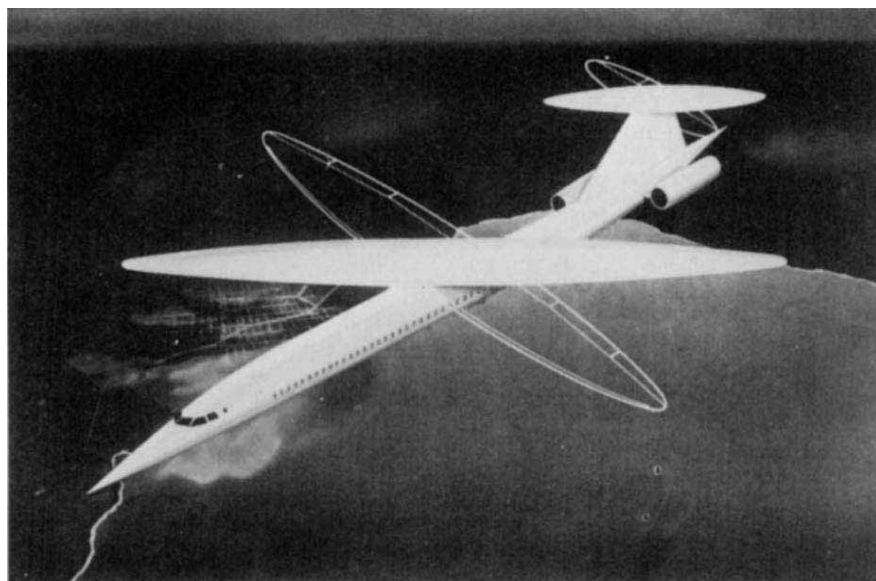
Both these pulsars seem to suffer discontinuous changes in period which can be explained in terms of starquakes—sudden changes in a relatively rigid rapidly spinning structure—but whereas the evidence favours a model for the Crab pulsar in which a solid crust "floats" on a core of neutron superfluid, in the case of the Vela pulsar it is more likely that the glitches arise from deformations of a solid core in a spinning neutron star.

This suggestion arises from the relatively large magnitude of the Vela spinups and their frequent occurrence, both of which require a reservoir of energy greater than that in the Crab pulsar.

Detailed changes in the periods of the two pulsars immediately following glitches also suggest that rather different mechanisms are operating in them.

What is the significance of this for pulsar models? Perhaps the principal conclusion to be drawn is that the Vela neutron star must be considerably more massive than the one in the Crab. Superfluid neutron matter solidifies only when its density exceeds 1.5×10^{15} g cm⁻³, which occurs in neutron stars of mass greater than $0.6 M_{\odot}$. So the Crab pulsar must have a mass less than this critical value, and the Vela pulsar must have a mass above $0.6 M_{\odot}$. More specifically, the observed changes in the Vela spin rate, together with the model put forward by Pines *et al.*, suggest a mass in the range 0.6 to $0.7 M_{\odot}$. And the possibility of "crustquakes" in the Vela pulsar is not ruled out, although these would probably occur independently of "corequakes" and only at intervals of several hundred years, making it unlikely that one will be observed in the near future.

AERODYNAMICS

Swing Wing or Swivel for SST?

R. T. Jones's concept of a supersonic aircraft with an asymmetrical wing. The wing outline shows the wing's position at take off.

ACCORDING to R. T. Jones, of NASA's Ames Research Center, the designers of supersonic transports such as Concorde may be mistaken in their tacit assumption that a symmetrically swept wing is the most efficient. Jones, who was one of the pioneers of the development of swept wing aircraft, now believes that a single oblique wing will be more efficient at supersonic speeds, because this produces less drag than the symmetrical swept wing which is now fashionable.

Any practical aircraft developed from these ideas would have to incorporate variable geometry, because the asymmetrical arrangement produces instabilities at low speeds. So Jones envisages a transport something like that shown in the figure here, which has a swivelling wing mounted on top of the fuselage. As well as the aerodynamic advantages of such an arrangement, the swivelling mechanism is simpler to construct, and imposes less strain on the wing, than the movable joints in conventional swept wing aircraft such as the ill fated F111.

In support of his idea Jones quotes the example of two conventional swept wing aircraft flying at supersonic speeds. If the aircraft fly side by side, the drag of each aircraft is twice that felt by a single aircraft; but if the aircraft fly in a staggered formation, with one slightly ahead of the other, the total drag on the pair need be no more than that on one such aircraft flying alone at the same speed.

This improvement in efficiency results from the favourable interference of waves along lines for which the normal

component of velocity is subsonic. Taking this to its limit, the wave drag of a long narrow wing tends to zero if the wing is swept behind the Mach cone, with the further advantage that by suitable choice of wing angle and speed cruising above Mach 1 can take place without any sonic boom being heard on the ground.

A great deal more work is necessary before even a prototype of this new kind of aircraft reaches the drawing board, let alone leaves it. But the attractions are, on paper at least, so great that it seems likely that progress will be pushed forward. With the wing in its conventional position, such an aircraft would require only one quarter of the take off energy of a comparable delta winged

aircraft, reducing runway requirements and takeoff noise. Once airborne, the wing would be rotated for cruising at up to Mach 1.2 with no associated sonic boom and with a fuel consumption comparable to that of present subsonic aircraft, and with the option of still faster speeds if sonic booms can be tolerated.

The only prototypes widely available at present are 9½ inch span asymmetrical flying wings made from balsa wood, supplied to journalists by NASA. In spite of its improbable appearance the model flew well when tested in *Nature* office and proved stable even at the low speeds appropriate for a glider of this kind.

MEMBRANES

A Case of Hydrophobia

from our Molecular Biology Correspondent

ONE does not have to swallow any of the more feverish theories about the nature of membrane proteins—such as that they are inverted globular proteins with the polar residues on the inside—to acknowledge that they must be possessed of structural attributes of their own. Many membrane proteins are insoluble, have strong aggregating tendencies, and are devoid of biological activity in the absence of phospholipids. It is doubtful whether these properties can be explained simply in terms of a superabundance of non-polar side chains, for the amino-acid composition is most often, at least on superficial inspection, unremarkable.

It is faintly surprising that an analysis of published data on amino-acid compositions has in fact only now been made. Capaldi and Vanderkooi (*Proc. US Nat. Acad. Sci.*, **69**, 930; 1972) have collected data for thirteen putatively constitutive membrane proteins, such as cannot be extracted without recourse to detergents or organic solvents,

Chloroplast Membranes and Photosynthesis

MUTANT strains of the green unicellular alga, *Chlamydomas reinhardi*, have been used to obtain significant information about the primary mechanism of photosynthesis. A great deal of this work has been carried out in Paul Levine's laboratory at Harvard University and in next Wednesday's *Nature New Biology* (June 7) Levine and two colleagues, Burton and Duram, outline some of their recent work involving this organism.

Levine *et al.* have investigated the nature of membrane polypeptides which can be isolated, using gel electrophoresis, from *Chlamydomas* and also from spinach chloroplasts. These polypeptides seem to consist of two fractions which are associated with either photo-

system one (S1) or photosystem two (S2). This was shown by using mutants deficient in S2 activity and then analysing spinach sub-chloroplast particles, rich in either S1 or S2 activity, treated with 'Triton' or digitonin.

It is not yet clear if all the polypeptide fractions are intimately involved in photosynthetic activity, but Levine and his colleagues show that at least three of the polypeptides, with molecular weights of about 30,000, are very specifically associated with S2. From studies with *Chlamydomas* mutants it seems that this specific relationship is with the reducing side of the S2 trap. Levine *et al.* are unable to say if the S2-associated proteins are acting in a structural or functional manner.

as well as six soluble membrane proteins, and have compared them with the compositions of 205 ordinary soluble proteins.

One may argue, of course, about the definition of a polar side chain, but Capaldi and Vanderkooi have taken the simplest view, and have included the charged and the hydroxylic aminoacids as well as glutamine and asparagine. Soluble proteins give a rather narrow distribution of contents of these polar side chains, the great majority lying between 40 and 53 per cent. Only thirteen of the proteins fell below this range. By contrast, the nineteen membrane proteins are evenly spread over a wide range of compositions, nine of them containing less than 40 per cent of polar side chains, five less than 37 per cent. The soluble membrane proteins are all in the range of composition of ordinary globular proteins. One can naturally not generalize about the extent of immersion of proteins in the membrane bilayer, though there is good evidence in one or two instances of proteins projecting right through it.

The dependence of membrane proteins on associated lipids for the stability of their functional conformation has been noted in several contexts. A related situation may be that in the serum lipoproteins, which undergo substantial, though reversible, conformational disorganization, when denuded of their complement of associated lipids.

An example of the effect on physiological activities of membrane enzymes of the extraction of phospholipids is to be found in the study of Mavis, Bell and Vagelos (*J. Biol. Chem.*, **247**, 2835; 1972). One approach, of course, would be to examine activities of isolated enzymes with and without associated phospholipids. Sandermann (*FEBS Letts.*, **21**, 254; 1972), for example, has found it possible to reactivate the membrane enzyme, C_{55} -isoprenoid alcohol phosphokinase of *Escherichia coli*, by adding lecithin. Three different lecithins gave a sharp maximum of activity at a mole ratio of about 800:1. The preparation of single membrane proteins in active form often presents insuperable difficulties, however.

The strategy of Mavis *et al.* is therefore to expose the membranes of *E. coli* to phospholipase C. Unlike phospholipase A, which has been used in earlier work, and which leads to the formation of surface active, and therefore potentially denaturing, lysophosphatides, this enzyme produces diglycerides, and phosphoryl compounds, which escape into the solution. The objective then is to relate the destruction of the phospholipids to the activity of various of the enzymes that are known to be lodged in the membrane. The phospholipase C digests the

phospholipids *in situ* at much the same rate as in the free state, though a fraction of the minor cardiolipin component resists hydrolysis. Three of the membrane enzymes — species with names of mystic power, that instantly inhibit the thought processes, such as acyl-CoA: glycerol-3-P acyltransferase — lose activity at the rate at which the phospholipids disappear. The species just mentioned is completely inactivated, and so also is one of the other two. The third suffers a partial inactivation to a limiting level of activity. Two other enzymes, both dehydrogenases, which are associated with the membrane, are unaffected. It seems possible that membrane-embedded enzymes are conformationally dependent on bound phospholipid or on the fluid, paraffin-like nature of their surroundings. Others perhaps are less deeply embedded, or even adhere to the surface.

At the same time, Kung and Henning (*Proc. US Nat. Acad. Sci.*, **69**, 925; 1972) report that succinate dehydrogenase, one of the species that is unaffected by digestion of the phospholipids, cannot be extracted from the membrane with a nonionic detergent. Two other dehydrogenases, by contrast, can be extracted and, moreover, in mutants that make them in large excess, are detected by enzymatic assay in the cell supernatant. The inference is that there is a limited number of specific sites on the membrane, which are fully saturated in the mutants. The soluble

enzymes apparently take no part in the operation of the respiratory chain. Moreover, the evidence is that several dehydrogenases may compete for the same membrane attachment sites.

A membrane-associated dehydrogenase from red cells has now been isolated by Carraway and Shin (*J. Biol. Chem.*, **247**, 2102; 1972). The most interesting feature of their work is that in intact cells this protein, which, as judged by detergent-gel electrophoresis has a subunit molecular weight of some 35,000, is the only one to be appreciably labelled by radioactive iodoacetate. The reagent is able to penetrate the cell, and therefore has access to a whole range of other proteins; in ghosts other components are in fact attacked, though the glyceraldehyde 3-phosphate dehydrogenase still remains prominent among them. Principally a single cysteine residue is modified.

What is also remarkable is that another thiol reagent, N-ethylmaleimide, shows no such selectivity, and is copiously incorporated into the whole complement of proteins. This shows once again the uncertainty regarding the factors that govern the susceptibility of protein side chains to modifying reagents. It suggests caution in interpreting results such as those of Dratz *et al.* in *Nature New Biology* last week (**237**, 99; 1972), who report that rhodopsin in intact or fragmented rod membranes, unlike the extracted protein, is not attacked by a reagent with high affinity for amino groups.

Induced Transplantation Antigen and Malignancy

OVER the past several years Harris and Klein and their various colleagues have exploited the technique of cell fusion mediated by inactivated Sendai virus to analyse malignancy. Their studies of the karyotypes and malignancy of hybrid cells obtained by fusing highly malignant cells with cells of low tumorigenicity, and the theories which have stemmed from such experiments, are widely known.

In next Wednesday's issue of *Nature New Biology* (June 7) Klein and Harris report their latest experiments with such hybrid cells and they conclude that the surface changes which lead in cells transformed by polyoma virus to the appearance of a virus-induced transplantation antigen are not alone sufficient to cause a cell to be malignant *in vivo*.

Cells transformed by polyoma virus are usually more malignant than their untransformed counterparts, and they also carry a new transplantation antigen which is presumably located at the cell surface and which is certainly virus-specific.

Whether or not this transplantation

antigen is coded for by the viral genome, however, remains an open question. Because the polyoma-induced transplantation antigen is exceptionally stable and, for example, survives strong negative selection, it has been suggested in the past by Klein, among others, that the presence of this antigen, or the surface change it reflects, may be an essential requirement for the proliferation *in vivo* of cells transformed by polyoma virus.

This seems not to be the case, however, for some clones of hybrid cells—produced by fusing malignant polyoma transformed cells with mouse fibroblasts of low tumorigenicity—prove in inoculation experiments involving syngeneic and semi-allogeneic mice to be of low tumorigenicity even though they retain the polyoma-induced transplantation antigen. Rather the malignancy of such hybrid cells seems to depend on the balance of chromosomes and therefore genetic determinants donated by the malignant and non-malignant parental cells; not until chromosomes from the non-malignant parental cell are lost does the hybrid become malignant.

EARTHQUAKES

Phase Changes

from our Geomagnetism Correspondent

LATE last year, Barton *et al.* (*Nature*, **234**, 293; 1971) drew attention to the possibility that some earthquakes may be the result of phase transitions at depth. The idea was certainly not new; but it has never gained much support, partly because it has not been easy to understand how a transition in the Earth's mantle could be rapid enough to produce the required shock.

As Barton and his colleagues pointed out, however, explosive phase transitions on a laboratory scale have been known to chemists since at least 1908, when Weston (*Chem. News*, **98**, 27; 1908) reported on the crystallization of sulphite from a supersaturated aqueous solution. More recent examples include the crystallization of nickel and bismuth from supercooled melts, the polymorphic transitions of antimony, arsenic, iron (austenite-martensite) and lead azide (β - α), and the polymerization of maleic anhydride.

Obviously, these particular reactions are not likely to take place in the Earth's mantle; but Barton *et al.* were simply pointing out that detonative phase transformations are not unknown and thus should not automatically be excluded as earthquake mechanisms. This is particularly relevant to deep earthquakes, for it is not entirely clear that under the conditions of temperature and pressure which obtain at, say, 700 km depth the conventional explanation in terms of shear phenomena is applicable.

Quite independently, Ringwood (*Earth Planet. Sci. Lett.*, **14**, 233; 1972) has now reconsidered the question of rapid phase transitions in the light of global tectonic processes. In the days when the mantle was regarded as static it was indeed difficult to visualize explosive phase transformations in the mantle because such transformations involve a change from a metastable to a stable state and it was not obvious how the required metastable state would arise in the first place.

In a dynamic mantle, on the other hand, there is no such problem. A sinking lithospheric slab may well be 1,000° C colder than the surrounding mantle, and this suggests that low pressure mantle minerals such as olivine may be carried down into the equilibrium field of the corresponding high pressure phases as long as the temperature of the descending slab does not exceed 700–800° C. In this way a metastable state may develop which, once induced to transform to the stable state, will do so in less than a second. In the mantle the phase change will take place in a volume of the order of cubic kilometres with the change

spreading throughout the volume accompanied by a shock wave. Barton *et al.* consider that the transition is induced by the shock wave and adds energy to it. Thus although the energy density is small, the total energy released is extremely large.

As for the geochemical nature of the phase transitions, much depends on the temperature in the descending lithospheric slab. At a depth of 400 km, this could easily be lower than 600° C, in which case metastabilities could arise for the basalt-eclogite and eclogite-garnetite transitions in the depth range 100–300 km, and for the olivine-spinel- β Mg₂SiO₄ and pyroxene-garnet transitions in the range 300–400 km. Ringwood considers that the temperature could easily be low enough to give metastability of β Mg₂SiO₄ and garnet also. But even if these transitions do not proceed sufficiently rapidly to produce earthquakes themselves, it still does not follow that no earthquake will result. A phase change may develop slowly; but the resulting contraction in volume could produce large stresses in the surrounding material and thus lead to secondary earthquakes by stress-induced failure.

The second traditional objection to phase transformations as deep earthquake sources is that the relevant seismic radiation patterns are more consistent with shear failure than with explosion mechanisms. Ringwood now largely discounts the importance of this point on the grounds that nuclear explosions also generate seismic waves with large shear components. The

explanation that nuclear explosions generate shear components by inducing failure in pre-existing stressed regions also applies to a sinking slab which, if descending under gravity, is stressed uniaxially.

The reason why a lithospheric slab descends under gravity in the first place is that the density of the slab is higher than that of the surrounding mantle—a density contrast which is produced largely by the presence of the two major phase transitions at 400 km and 650 km. The alternative view, that a nuclear explosion produces shear waves because of failure in the bomb-cavity wall, also applies to phase transformations by way of the secondary earthquake mechanism mentioned earlier.

INSTRUMENTS

Scanning Microscopy

from a Correspondent

THE pattern of the fifth annual scanning electron microscopy symposium, which was held in Chicago on April 25 and 26, was different from previous years in that there were no contributions dealing solely with the application of scanning microscopy. Instead, several interesting papers stressed the analytical capabilities of scanning electron microscopes (SEM) and clearly pointed the directions in which instrumentation development is proceeding.

Dr K. C. A. Smith (University of Cambridge), who opened the instrumentation session, while noting the high resolution capabilities of field emission

Q β Replicase as a "Repressor"

IN *Nature New Biology* next Wednesday (June 7), Weissman and his several associates report another step in their most impressive analysis of the replication of the single stranded RNA genome of coliphage Q β . This is a direct sequel to the work of Kolakofsky and Weissman (*Nature New Biology*, **231**, 42; 1971) which showed that the Q β replicase acted as a "repressor" of the translation of the Q β coat protein cistron and hence of the succeeding replicase-subunit cistron. They found that Q β replicase, by binding to Q β plus strand RNA, blocks the binding of ribosomes to the initiation sequence at the start of the coat protein cistron and hence prevents the initiation of synthesis of further coat protein molecules without, however, stopping the completion of synthesis of coat protein molecules already started.

Pursuing this lead Weissman's group have now isolated the internal fragment of Q β RNA to which the replicase binds by allowing replicase to bind to Q β RNA molecules labelled for different

extents from the 5' end of the molecule and then digesting the complex with ribonucleases. This procedure, derived from that developed by Steitz to isolate RNA phage ribosome binding sites, depends on the replicase molecule protecting from nucleolytic attack that part of the Q β RNA to which it is bound. Once isolated the protected fragment, which must include the replicase binding sequence, was then sequenced by conventional methods.

Weissman and his colleagues found that the fragment protected by the replicase includes the ribosome binding site of the coat protein cistron up to the initiation triplet AUG. It seems therefore that Q β replicase acts as a "repressor" of gene expression by directly competing with ribosomes for the initiation site of the coat protein cistron. Moreover, the replicase probably binds at this position before it interacts with the 3' end of the Q β plus strand RNA which serves as template for the initiation of synthesis of the 5' end of a complementary minus strand.

guns, clearly demonstrated the potentialities of some of the new boride emitters which operate under less stringent vacuum conditions. Looking forward for the next ten years, Dr Smith mentioned improvements in signal detection and the presentation of the final image, and gave some examples of the dynamic capabilities of the SEM. There then followed papers which described SEM at 8 K and at 1,600° C, and Dr H. Yakowitz (National Bureau of Standards, Washington) gave details of a new high contrast cylindrical secondary electron collector which improves the signal quality from highly irregular surfaces.

The storage-display system described by Drs C. J. D. Catto and K. C. A. Smith (University of Cambridge) shows great promise, particularly where labile specimens susceptible to beam damage are being examined. Dr A. Boyde (University College, London) examined the problems associated with "real-time" three-dimensional examination of SEM images, a prerequisite for any meaningful analysis of morphological data.

The afternoon session, on information analysis, included papers on the X-ray microanalytical capabilities of SEM. It was made clear by Drs L. V. Sutfin (Harvard Medical School) and J. C. Russ (Edax Laboratories, N. Carolina) that high resolution X-ray microanalysis studies may be carried out on a wide range of materials, and further improvements may be expected in the sensitivity of energy dispersive X-ray detectors. The session also contained papers on crystallographic contrast, electron beam induced current in semiconductors and a review of the basic limitations of probe forming systems.

The second day started with a session on some of the techniques used in SEM. Dr P. Echlin (University of Cambridge) reviewed the present methods of thin-film deposition onto non-conducting samples, and suggested a number of new coating techniques whereby more information could be obtained from specimens examined in the SEM. Dr J. Pawley (University of California at Berkeley) and Dr G. E. Pfefferkorn (University of Münster) considered the problem of surface charging, and there were several contributions on the nature of the electron beam interaction with the specimen. Dr E. K. Brandis (IBM, New York) described techniques for measuring height and depth, and the session finished with some of the specialized techniques required for the examination of radioactive material and semiconductors.

The final session was concerned with the physical limits of the scanning transmission microscope, and several speakers dealt with the theoretical capabilities of these instruments. Dr

J. M. Cowley (Arizona State University) described some of the design features of his 1 MeV SEM, though the anticipated preliminary results from this instrument were not produced.

After the two day meeting there was a one day workshop sponsored by the National Institutes of Health, Washington, which concentrated on the problems of biological specimen preparation for SEM. The session was introduced by Mr E. Bynum (National Institute of General Medical Sciences), who spoke about the increased interest in scanning electron microscopy by the NIH and indicated that the agency is likely to increase its support for the medical and biological application of this type of instrumentation. After an excellent review by Dr A. Boyde (London), which dealt chiefly with the more conventional means of chemical fixation, there followed a series of contributions which demonstrated the advantages of cryobiological methods for specimen preparation. Problems of freezing and freeze drying were dealt with by Drs

A. P. MacKenzie (Cryobiology Research Institute, Madison) and R. Thalmann (Washington University, St Louis), and Dr M. Nemanic (University of California, Berkeley) gave information relating to critical point drying and cryofracture. Much of the day was spent in detailing methods for particular specimens, and the papers on living tissue (Dr J. A. Nowell, University of California, Davis), plant material (Dr B. J. Panessa, New York University) and skin (Dr G. E. Pfefferkorn, Münster) were of particular interest.

Many manufacturers displayed their machines at the meeting, and it is noteworthy that most commercial instruments are now guaranteed better than 100 Å in the secondary mode of operation. Some of the companies ran seminars in conjunction with the meeting, and the one day seminar by Kent Cambridge which preceded the meeting was particularly useful in demonstrating the wide range of analytical accessories which may be fitted to modern scanning microscopes.

Control of Serotonin Synthesis in Rat Brain

UNDERSTANDING of the biochemical mechanisms associated with the synthesis, storage and release of the catecholamine neurotransmitter substances has increased rapidly in recent years. Much less is known, however, about the biochemistry of the other monoamine transmitter, 5-hydroxytryptamine (5-HT) (serotonin).

The way in which the rate of biosynthesis of the catecholamines noradrenaline and dopamine is controlled by nerve activity is now quite well defined. Such regulation seems to involve the biochemically familiar mechanism of end product feedback inhibition, in which the catecholamines control the first and rate-limiting step in their own biosynthesis, and the conversion of L-tyrosine to L-DOPA catalysed by the enzyme tyrosine hydroxylase. A report by Hamon *et al.* in next Wednesday's *Nature New Biology* (June 7) now provides evidence in favour of a similar mechanism to control the rate of biosynthesis of 5-HT in serotonergic nerves in the rat brain.

Such a mechanism had been suggested previously by workers from this group on the basis of results obtained in the intact animal (Macon *et al.*, *J. Neurochem.*, **18**, 323; 1971). In these experiments a decreased conversion of radioactively labelled tryptophan to 5-HT in the brain was observed after treatment of rats with inhibitors of the enzyme monoamine oxidase to cause an increase in the steady state concentration of 5-HT. Because of the complexities involved in such experiments in the intact animal, however, it is reassuring

to see that in Hamon *et al.*'s study similar effects could be observed in an isolated tissue preparation.

A reduced rate of conversion of labelled tryptophan to 5-HT was observed in slices of brain obtained from animals treated a few hours previously with pargyline or pheniprazine (monoamine oxidase inhibitors) when the tissue was incubated in saline medium containing the labelled amino-acid. Under such conditions, when the concentration of 5-HT in the tissue had increased to approximately 2.5 times the normal level, 5-HT formation was reduced by about 40 per cent. Evidence that this reduction in 5-HT formation might be related to an increase in endogenous indoleamine concentration was confirmed by other experiments.

It thus seems that 5-HT may regulate its own biosynthesis by some form of end product inhibition. The precise nature of this inhibitory effect of 5-HT remains to be elucidated, but it does not seem to involve the rate of transport of the precursor amino-acid tryptophan into the serotonergic neurones, because the presence of elevated concentrations of 5-HT did not reduce the specific activity of the tryptophan pool in Hamon *et al.*'s studies.

Hamon *et al.* conclude that the most probable mechanism is an inhibition of the first step in the biosynthetic pathway, the conversion of tryptophan to 5-hydroxytryptophan, catalysed by the enzyme tryptophan hydroxylase. Such a conclusion must now await confirmation by studies with the isolated purified enzyme.

TUMOUR VIRUSES

New Assays

from our Cell Biology Correspondent

ASSAYS which allow the experimenter to measure the number of infectious units in a stock of virus particles are a *sine qua non* of quantitative virology. For that good reason alone the reports of Kawai and Hanafusa and of Kelloff, Hatanaka and Gilden in the current issue of *Virology* (48, 126 and 266; 1972) are bound to attract widespread attention, for both groups have devised new ways of assaying the infectivity of certain RNA tumour viruses. Kawai and Hanafusa's experiments are particularly interesting because they have hit upon a rapid and comparatively simple plaque assay for at least some strains of avian leukosis virus.

Most strains of avian leukosis virus infect and replicate in chick embryo fibroblasts without either transforming the cells or causing anything more than an occasional and transient cytopathic effect. As a result these viruses have to be assayed by comparatively laborious methods based on viral interference, viral antigenicity or on the fact that the avian leukosis viruses act as helpers for the replication of certain Rous sarcoma viruses, which can be assayed because they transform cells and give rise to foci or transformants.

Until recently much the same could be said of assays for murine leukaemia viruses, but in 1970 Rowe *et al.* devised the now widely used XC cell plaque assay for mouse leukaemia viruses, which is ingeniously based on the interaction between mouse cells infected with mouse leukaemia virus and rat XC cells transformed by the Prague strain of Rous sarcoma virus. The assay which Kawai and Hanafusa have devised for some avian leukosis viruses in some respects resembles Rowe's XC cell assay.

Kawai and Hanafusa find that when lawns of chick embryo cells infected by a temperature sensitive mutant of Rous sarcoma virus (ts-68) are held at the non-permissive temperature (at which they do not have a transformed cell phenotype) and superinfected with avian leukosis viruses of subgroups B and D (RAV 2, RAV 50 and AMV 2), plaques develop within 4 days as the leukosis viruses kill their hosts. Moreover, estimates of virus infectivity obtained in this way are very close to those obtained by conventional interference assay. By contrast the strains of avian leukosis viruses of subgroups A, C and E that have so far been tested do not cause plaques to develop and cannot therefore be assayed at present by this procedure.

The results of several control experiments prove that the plaques which develop after the ts-68 transformed cells

have been superinfected with RAV 2 preparations are indeed the result of RAV 2 infection and not the result of infection by some other agent contaminating the RAV 2 stocks. For example, plaque formation can be prevented by neutralizing the RAV 2 stocks before they are used for superinfection with anti-RAV 2 antisera or by overlaying the lawns of ts-68 transformed cells after infection with RAV 2 with sloppy agar containing this antiserum.

Why only certain strains of avian leukosis viruses are able to form plaques in these conditions and how the plaque forming leukosis viruses kill their hosts remain to be elucidated. When answers to these questions are forthcoming it may be possible to arrange conditions such that leukosis viruses of subgroups A, C and E can also be plaque assayed.

The assay which Kelloff *et al.* have devised is apparently of general use for the measurement of infectivity of any C type RNA tumour virus. It is based on calibrating the amount of reverse transcriptase activity in the supernatant media of cultures of cells infected by these viruses with the amount of the progeny virus present and the infectivity of this virus as assayed by the so-called COMUL and COHAL assays. At least with the several viruses that Kelloff *et al.* have used the enzymatic assay gives 9 days after infection an estimate of the number of infectious virus particles in such supernatants comparable to those obtained at 21 days by existing assays. The new assay, if it proves as

reliable as these preliminary results suggest, should at least be a valuable time saver.

SELENOLOGY

Luna 20 Samples

from our Soviet Correspondent

PRELIMINARY analysis of the lunar rock recovered by the Luna 20 spacecraft has revealed the presence of more than seventy elements. Comparison with the Mare specimens recovered by Luna 16 from a site some 120 km away from where the Luna 20 specimens were obtained shows that there is less iron oxide but a small increase in the proportion of alkalis in the Luna 20 specimens (*Pravda*, May 4; 1972).

Component	Rock fraction			
	Basalt Luna 16	Anorthosite olivine Luna 20	Soil	
			Luna 16	Luna 20
SiO ₂	42.95	42.40	41.90	44.40
Al ₂ O ₃	13.88	20.20	15.33	22.90
FeO	20.17	6.40	16.66	7.03
CaO	10.80	18.60	12.53	15.20
MgO	6.05	12.00	8.78	9.70
TiO ₂	5.50	0.38	3.36	0.56
Na ₂ O	0.23	0.40	0.34	0.55
K ₂ O	0.16	0.52	0.10	0.10

A comparison of the percentages of the chief components of the Luna 20 specimens with the revised figures for Luna 16 (which differ slightly from those quoted in *Nature* (228, 492; 1970)) is given in the table.

Codon-anticodon Specificity

THE nucleotide sequences of another pair of tRNAs from *Escherichia coli*—those which translate the two glutamine codons CAA and CAG—are reported in *Nature New Biology* next Wednesday (June 7) by Folk and Yaniv who add their voices to the growing speculation that the replacement of uridine by 2-thiouridine in anticodons may be an evolutionary device for preventing mis-coding. Apparently Folk and Yaniv intend ultimately to characterize the nature of a new suppressor mutation of *E. coli*, *su₇*, which leads to the translation of the UAG amber codon as glutamine. Preparatory to that they decided to sequence the two glutamyl tRNAs in wild type *E. coli*, tRNA^{Gln}₁ which is specific for the codon CAA and tRNA^{Gln}₂ which is specific for the CAG codon.

As expected, the sequences of these two tRNAs can be drawn as a clover leaf structure and they are very similar indeed, differing only by seven nucleotides, which suggests to Folk and Yaniv that the genes for these two tRNAs may during evolution have diverged from

duplicate copies of a single gene.

Of the seven nucleotide differences, six occur in double-stranded stem regions of the molecule; the seventh, however, is of much greater interest, being at the 5' position of the presumptive anticodon. In tRNA^{Gln}₂, which is specific for the codon CAG, the anticodon sequence is CUG as expected from the rules of base-pairing. In the tRNA^{Gln}₁ molecule, however, which reads the CAA codon the anticodon sequence NUG.N is almost certainly 2-thiouridine or a derivative in which the oxygen atom at position 2 in the uridine molecule is replaced by a sulphur atom.

The selective advantage of this change becomes apparent when possible "wobble" base pairing is taken into account. Uridine at this position could base pair with A but also by wobble with G or U. But 2-thiouridine is much less likely to form these wobble base pairs and so the chances of the CAA specific glutamine tRNA translating the CAG glutamine codon or the CAU histidine codon are reduced.

New Light on the Origin of Birds and Crocodiles

ALICK D. WALKER

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Detailed evidence from the skull of *Sphenosuchus*, and from embryological and other resemblances between birds and crocodiles, suggests that these two groups are much more closely related than has been realized.

It has long been recognized that birds are more closely related to crocodiles than to other living reptiles. Many features of the anatomy of the two groups testify to this relationship—for example, the structure of the heart in crocodiles represents an advanced stage on the way to the bird type, by contrast with the more primitive conditions found in other living reptile groups¹. In the skull, also, the ossified laterosphenoids and the occurrence of mandibular and antorbital fenestrae (the latter present in early crocodilians) provide further evidence of affinity. These and other resemblances are usually assumed to be the result of independent descent from the Thecodontia, a group of Triassic reptiles which is also believed to have given rise to the two orders of dinosaurs and the flying reptiles, and which, largely by inference, is thought to have possessed all these features. But detailed study and dissection of bird and crocodilian skulls, and comparison with that of *Sphenosuchus*, a crocodile from the Upper Triassic Red Beds of South Africa, throws doubt on this basic assumption and suggests, on the contrary, that birds and crocodiles form essentially one stock.

Probably the most important feature of the crocodilian skull, and one which has received little attention, is the forward position of the primary or original head of the quadrate. This has moved forward and inward along the paroccipital process so that it no longer articulates with the opisthotic, and is prevented from doing so by a process from the prootic. Thus the head of the quadrate in the Crocodylia articulates essentially with the squamosal and prootic, whereas in all other archosaurs (thecodonts and their descendants, Fig. 1a), it rests in a cup formed by the squamosal and opisthotic and does not touch the prootic. That this is an ancient condition in the "orthodox" crocodilian stock is demonstrated by the skull of a small protosuchian crocodile from the Red Beds of Lesotho, BM(NH) R 8503, in which the suture between prootic and opisthotic is clearly visible passing behind the head of the quadrate. The condition in birds (Fig. 1c) is basically the same as that in crocodiles, but two principal factors have conspired to conceal this fundamental resemblance—first, the great development of secondary contacts between the crocodilian quadrate and the braincase, virtually blocking the cranio-quadrate passage, and, second, the inflation of the bird cranium because of the increased size of the brain, which has forced the upper articulation laterally, obscuring the fact that it is morphologically more medial and anterior than in typical reptiles. To some extent bone added to the exoccipital in birds has also enclosed some of the nerves and vessels in a rather crocodilian manner.

The inward and forward movement of the quadrate head is very clearly seen in *Sphenosuchus* (Fig. 1b), in which there were, as yet, no firm connexions with the side of the brain-

case. It is difficult to visualize the migration of the upper quadrate articulation taking place otherwise than at a time when the bone was freely movable (that is, streptostylic), and this assumption is borne out by the complete absence in *Sphenosuchus* and other crocodiles of the descending process from the squamosal which, in typical archosaurs, meets the quadratojugal halfway down the lateral temporal fenestra. The loss of this stabilizing process in lizards, also in the context of the development of streptostyly, has been pointed out by Robinson².

One of the most striking characteristics of the bird skull is precisely this streptostylic quadrate (Fig. 2a), part of a kinetic system whereby forward movement of the lower end of the bone operates a "push rod" arrangement of bony bars which has the effect of elevating the beak; upward bending takes place at a region of flexible bone in front of the orbits. The whole palate and beak are thus movable relative to the cranium. There are indications that a similar, but more primitive streptostylic-kinetic system had only recently ceased to operate in the skull of *Sphenosuchus* (Fig. 2b). The head of the quadrate rests for the most part in a smooth socket, but over a small area it is fused to the squamosal. The upper part of the quadratojugal has only smooth groove-and-tongue contacts with the squamosal and quadrate and has every appearance of being a new extension, recently evolved to prevent streptostyly. Other such devices are present in the skull—thus the quadrate and pterygoid form a deep lamina in close proximity to the side wall of the braincase, and the basal articulation of the pterygoid is immovably fixed. The reason for this loss of kinetism is, with little doubt to be found in the size and powerful build of the maxilla and its dentition, and it seems reasonable to infer that the nasals and certain other bones have, in consequence, been thickened. It is thus not easy to be certain of all the former positions of relative movement. A pivot joint with the quadrate is still present within the lower end of the quadrato-jugal, however, and bending probably formerly occurred at a depression and change of slope on the nasals in a position comparable with the hinge in the bird skull. The fourth pivot is most likely to have been at the overlap between jugal and maxilla. Sliding was still clearly possible at bone overlaps as shown in Fig. 2. Some flexibility of the postfrontal/jugal bar may also have been necessary; significantly, this is the most slender of all the "arches" at the postulated position of bending.

A kinetic system of this type would necessarily involve movement of the entire palate by way of the pterygoids. The bird palatine is extremely elongated, the anterior end passing below a short shelf from the maxilla (Fig. 3b) to reach the premaxilla. It is thus able to transmit the forward thrust from the quadrate and pterygoid in a direct line to the beak. Significantly, the anterior end of the palatine in *Sphenosuchus* (Fig. 3a) is elongated along the outside of the choana and it meets the maxilla very low down, below the level of the short secondary palate formed by the maxillae, concealing the interdental plates and almost touching the teeth. It was thus well placed to transmit the forward push from the pterygoid because it has the maximum possible moment-arm about the nasal hinge (Fig. 2b). If kinetism had continued, the anterior end of the palatine would have tended to migrate forwards, as this would bring the direction of thrust more nearly parallel to the mid-line.

In doing so it would have passed below the maxillary shelf. The long axis of the palatine is further forward, and less oblique to the mid-line, than that of the ectopterygoid, and would thus tend to supplant the latter, a process which has led to the loss of the ectopterygoid in birds.

Furthermore, the *Sphenosuchus* palatine has a short, almost vertical trough at the rear end of the choana, supported by a branching ridge-system (Fig. 4), the whole bearing a striking resemblance to that which occurs in birds. The fulmar (*Fulmarus glacialis*) and other petrels, which are commonly regarded as among the most primitive of non-ratite birds, show especially close similarity in this respect, and to some extent in the vomer also. In birds,

Preparation of the better preserved right side gives a more accurate impression of the build of the occiput and adjacent regions (Fig. 5). The bird-like construction of this part of the skull then emerges, although it was not at first recognized. Among the forms studied the closest resemblances are with the divers (Gaviiformes), the Procellariiformes, particularly the petrels, the penguins (Sphenisciformes), and the auks and gulls, particularly the razorbill, *Alca torda*. The first three of these groups, and sometimes the auks also, are placed as the most primitive of living carinate birds in taxonomic lists. There are great differences between *Sphenosuchus* and the birds because of the greater relative height of the occipital plate compared with that of the

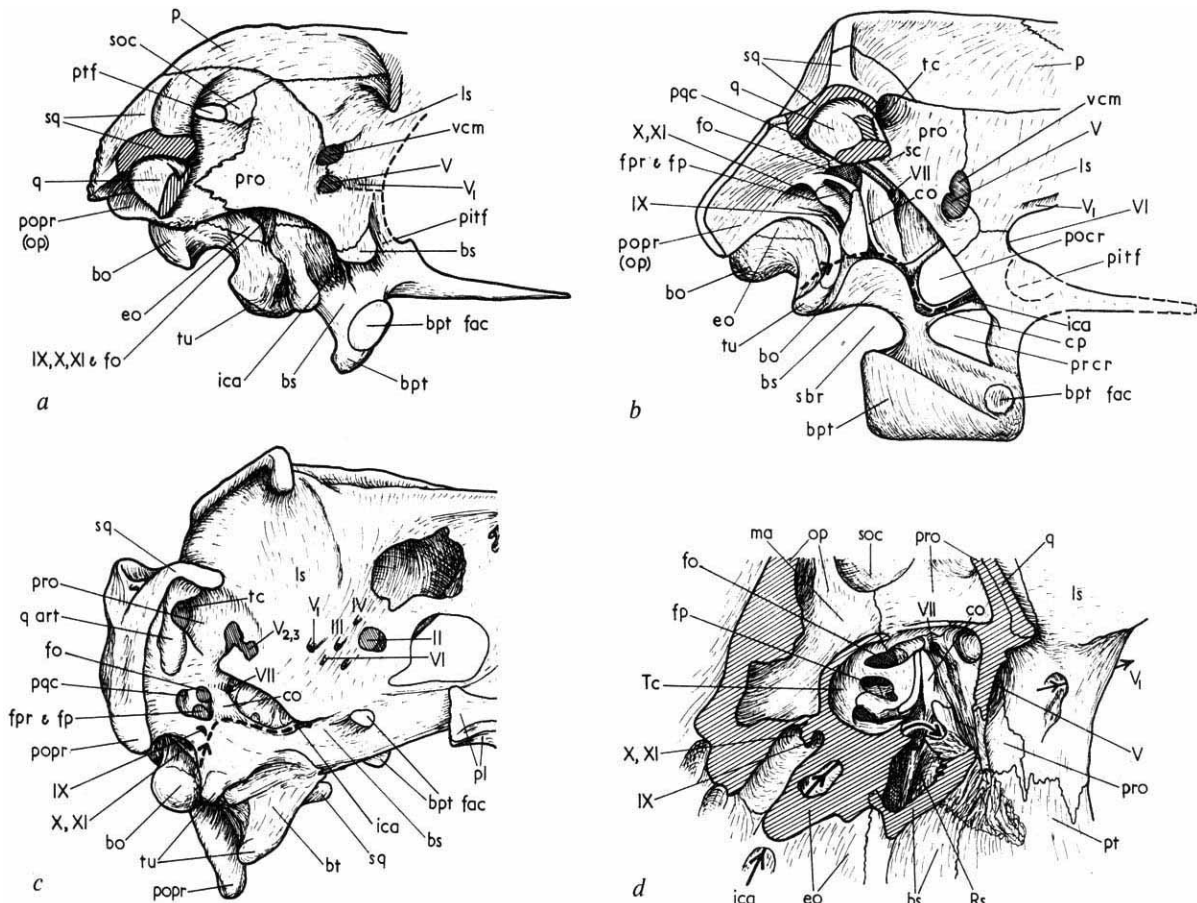


Fig. 1 *a*, The typical archosaurian upper quadrate articulation in the Upper Triassic thecodont *Stagonolepis*³. The paroccipital process is inclined too strongly backward, because of distortion ($\times 5/8$); *b*, preliminary restoration of the right side of the braincase of *Sphenosuchus*, with distortion corrected ($\times 1$). (Each basiptyergoid process is suspended from separate anterior and posterior struts.) *c*, Oblique lower right view of rear part of skull of giant petrel, *Macronectes giganteus*, quadrates and pterygoids removed ($\times 1.2$); *d*, parasagittal section through the right tympanic cavity of *Gavialis* ($\times 1$). As the fenestra pseudorotunda is nearly perpendicular to the plane of the section it cannot be indicated. Abbreviations as in Romer³⁷, with the addition of bpt, basiptyergoid process; bpt fac, basiptyergoid facet; bt, basitemporal; co, cochlear prominence; cp, carotid pillar; fo, fenestra ovalis; fp, foramen perilymphaticum; fpr, fenestra pseudorotunda; ica, canal for internal carotid artery (course indicated by heavy line); ma, mastoid antrum; pitf, pituitary fossa; pocr, postcarotid recess; popr, paroccipital process; pqc, post-quadrate canal (posterior tympanic recess); prcr, precarotid recess; q art, quadrate articulation; Rs, rhomboidal sinus; sbr, sub-basisphenoid recess; sc, canal for stapedial artery; Tc, tympanic cavity; tc, temporal canal (superior tympanic recess); tu, basioccipital tuber; vcm, middle cerebral vein.

the palatines articulate with the base of an ossified inter-orbital septum, but it is not difficult to derive this condition from that of *Sphenosuchus*, in which they extend medially above the pterygoids and probably met the cartilaginous interorbital septum, much as in *Stagonolepis*³.

Previous figures of the rear part of the skull of *Sphenosuchus*^{4,5-7} have been based on the left side in which, however, the paroccipital process has been bent upwards.

quadrates, and also because of the large size of the cerebellar prominence and the inclusion of the proatlas in the occiput in the latter group. Important resemblances are, however, to be seen in the crescentic shape of the occipital surface, with short downward and backward directed paroccipital processes which project well behind the quadrates and form posterior walls to the tympanic cavities; the contours of the occipital surface with its pattern of low ridges and

shallow depressions, the outer one of which vees forward above the head of the quadrate; and the thin transverse crest with its paired dorsolateral culminations. The well-developed sagittal crest between large temporal fossae, and the orientation of the transverse crest in the vertical plane make for a particularly close resemblance to the Great Northern Diver (*Gavia immer*).

The generally accepted interpretation of the postorbital region of the bird skull is that it has been derived from a normal diapsid reptilian type, that is, with both an upper and lower temporal fenestra, by the breakdown of the bar between the two fenestrae and also of that separating the lower one from the orbit. The skull of *Sphenosuchus* seems to bear out this interpretation, and the upward and forward curvature of the upper temporal bar agrees with the relative positions of the relics of this bar in the bird skull. The assumption of some former movement at the postorbital bar provides an explanation for its loss during the perfection

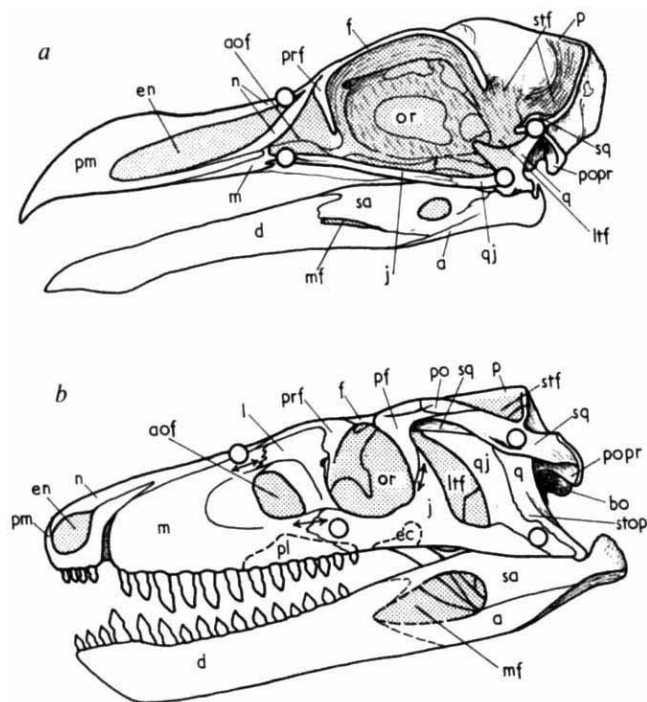


Fig. 2 Side views of skulls of *a*, Razorbill, *Alca torda* ($\times 1$); *b*, *Sphenosuchus acutus* ($\times 0.4$). $\circ$, Pivots or former pivots; ---, positions of palatine and ectopterygoid contacts within maxilla and jugal; stop, lateral stop on quadrate limiting backward movement; aof, antorbital fenestra; ltf, lateral temporal fenestra; mf, mandibular fenestra; stf, supra-temporal fenestra.

of the kinetic apparatus, as its retention would only have been a hindrance to movement of the jugal. At some quite short antecedent stage in the ancestry of *Sphenosuchus* the upper part of the quadrate seems to have been remarkably free of the adjacent bones, as it is in birds. The prefrontal in the fossil sends a lateral process far down within the lachrymal. As the latter bone is essentially superficial to the prefrontal, and appears to have had a sliding contact with the maxillo-jugal bar, its presence in a kinetic skull would have become largely superfluous, so that it is not surprising that it has been lost from the skull of modern birds, leaving the lower end of the prefrontal ligamentously attached to the jugal and the lachrymal duct lying in a groove at the side of the prefrontal (Fig. 5c, f).

One of the most unexpected features of the skull of *Sphenosuchus* is that there seem to have been paired salt-excreting glands essentially like those of living sea-birds^{8,9},

but relatively smaller, situated in shallow depressions on the upper surfaces of the frontals. The blood supply to these glands passed round the upper border of the orbit by four or five marginal grooves and foramina. The duct of the gland evidently entered the nasal chamber by means of a foramen situated, as in many birds, between frontal and prefrontal (Fig. 5c, f). Well developed salt glands were

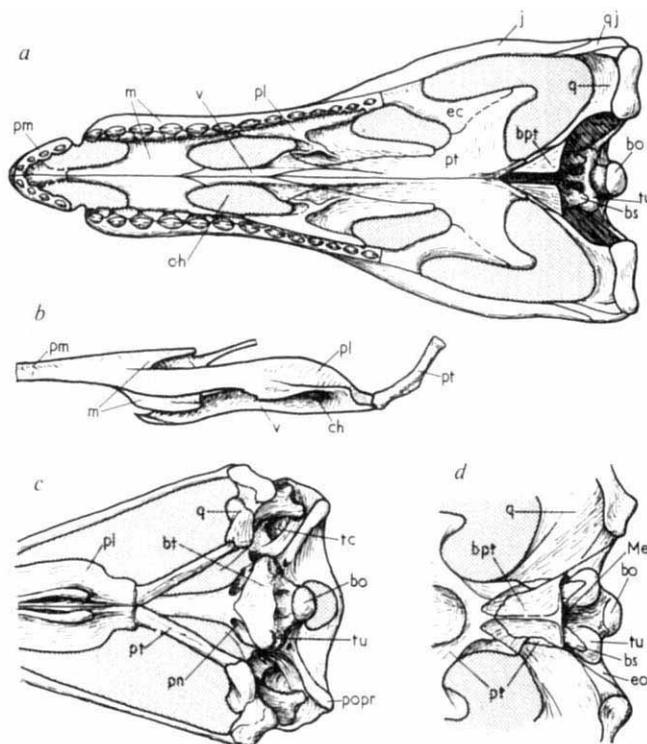


Fig. 3 *a*, Palatal view of skull of *Sphenosuchus* ($\times 0.46$); *b*, oblique lower view of left half of palate of Fulmar, *Fulmarus glacialis* ($\times 0.92$); *c*, lower view of rear part of skull of Wandering Albatross, *Diomedea exulans* ($\times 0.46$); *d*, same of the Lower Jurassic crocodile *Pelagosaurus typus* ($\times 0.55$), from BM (NH) No. 32599. ch, Choana; pn, pneumatic foramen communicating with precarotid recess; Me, median eustachian aperture, which opens directly above into a pair of sinuses within the basioccipital.

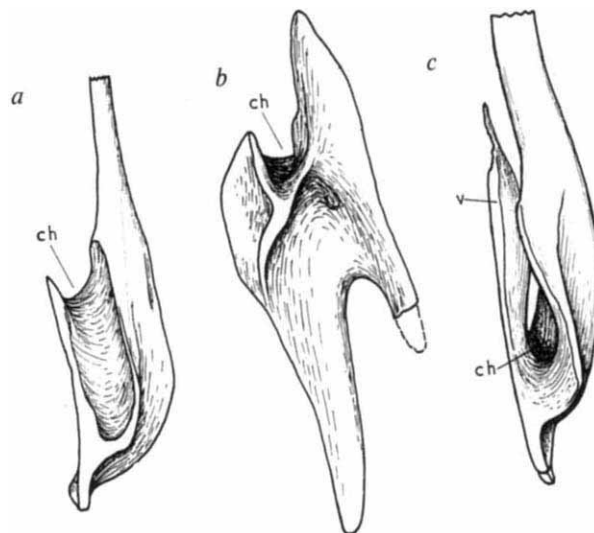


Fig. 4 Palatal views of left palatines of *a*, Razorbill, *Alca torda*, slightly oblique, anterior (upper) end incomplete ($\times 2$); *b*, *Sphenosuchus*, medial border (on left) incomplete, hidden by pterygoid ($\times 1.3$); *c*, *Fulmarus glacialis*, oblique, including also part of vomer ($\times 2$).

present in the Upper Cretaceous birds, *Ichthyornis* and *Hesperornis*¹⁰, but they have usually been regarded as having no more than adaptive significance for bird phylogeny; it may be, on the other hand, that their reduction or absence is the specialized condition.

Basipterygoid Processes

Perhaps the most significant feature, however, ranking close to the position of the quadrate articulation in importance, is the nature of the basipterygoid processes. These are very large structures, with almost flat medial surfaces only a few millimetres apart (Figs. 1b, 3a, 5a). They consist of delicate and incompletely ossified shells of bone, perhaps surrounding an inner cartilaginous core, and they were certainly at least partly pneumatized because they communicate freely above with the other air spaces in the skull. Both bird and crocodile skulls have a complex series of such air spaces which communicate with the middle ear cavity and the roof of the mouth, although they differ in detail. No serious attempt seems to have been made to determine the homologies of the various branches of the system between the two groups. This can now be done by the intermediary of *Sphenosuchus*, in which the skull is very extensively pneumatized (Fig. 1b); only two of the more

further back to join up with the lower surface of the rear end of the basisphenoid, leaving only a single aperture facing backwards, a state of affairs like that in the primitive Lower Jurassic crocodile *Pelagosaurus* (Fig. 3a, d) would have been attained. In the early growth stages of *Crocodylus cataphractus*, cartilaginous infrapolar processes from the basitrabecular regions grow back below the basal plate, and Müller¹¹ has shown that they ossify as the anterior part of the basisphenoid and thus form the anterior and ventral boundary to the median part of the eustachian system. Thus it seems that in this respect the embryonic development recapitulates phylogenetic history.

In well pneumatized bird skulls the internal carotid arteries generally run in bony tubes or grooves, which describe sigmoid curves along the sides of the cranium and pass upward in front to join just before entering the pituitary fossa. In their anterior course they are surrounded by an air space lying below the basisphenoid and above a triangular sheet of bone called the basitemporal plate (Figs. 1c, 3c). In *Sphenosuchus* the basisphenoid, like that of birds, consists of thin sheets of bone penetrated by large pneumatic spaces. The internal carotids ran in thin-walled bony canals formed by the floor of a large air space (Fig. 1b, pocr) and turned upward in grooves on either side of a tapering pillar of bone to enter the base of the pituitary fossa. The similarity

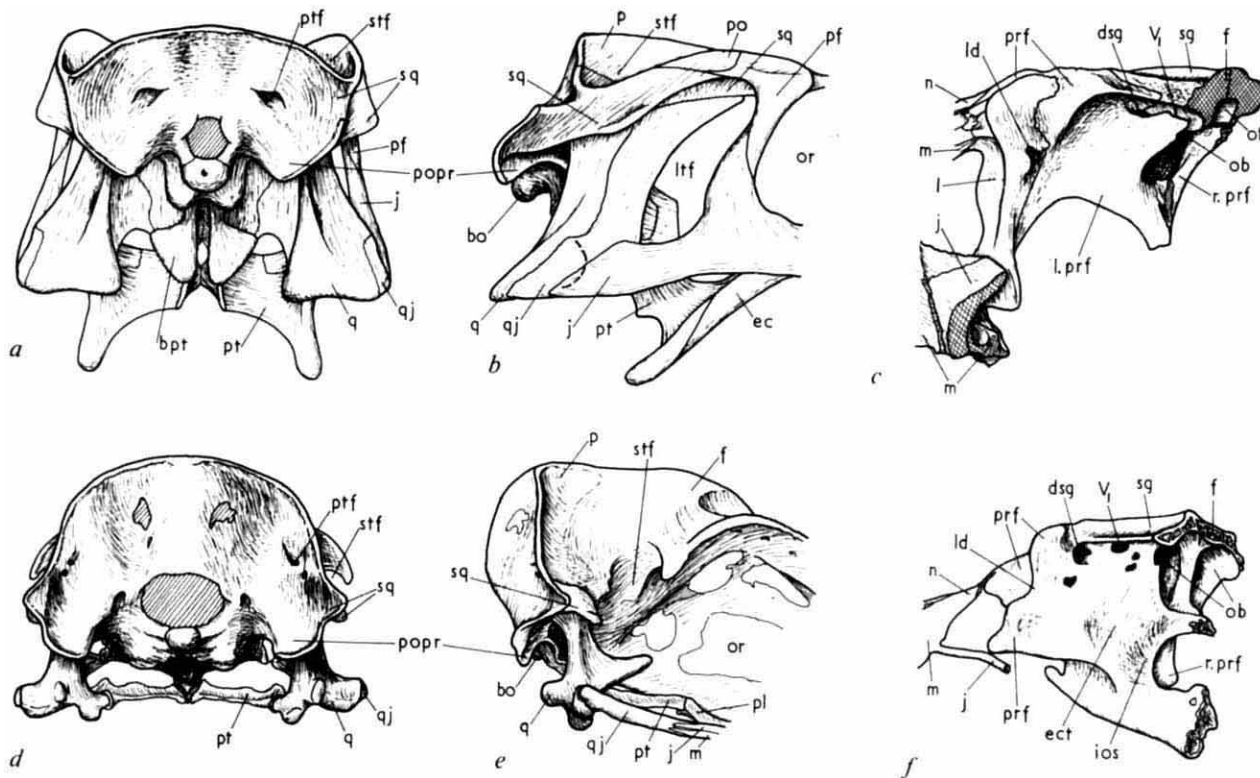


Fig. 5 a, Occipital view; b, right lateral view of postorbital part of skull, and c, posterolateral view of left prefrontal region of *Sphenosuchus*. Occipital sutures omitted in a, broken line in b indicates position of pivot within quadratojugal. d, e, Comparable views of skull of razorbill, *Alca torda*; f, posterolateral view of left prefrontal-ectethmoid region of *Fulmarus glacialis*. In c the posterodorsal process of the prefrontal has been displaced sideways by distortion; it originally lay below, but not touching, the flange at the side of the olfactory tract. a, b: $\times 0.6$; c, $\times 0.85$; d, e, $\times 1.4$; f, $\times 1.5$. dsq, Foramen for duct of salt gland; ect, ectethmoid; ios, interorbital septum; ld, position of lachrymal duct; ob, position of olfactory bulbs; ot, channel for olfactory tracts; ptf, post-temporal fossa; sg, depression for salt gland; V₁, notch or foramen for ophthalmic branch of trigeminal nerve.

important conclusions can, however, be outlined here.

There are good comparative anatomical reasons for thinking that the large hollow above and behind the basipterygoid processes (sbr in Fig. 1b) is equivalent to the median ventral portions of the eustachian (pneumatic) system of modern crocodiles. If the processes coalesced ventrally and grew

of this arrangement to that of the bird is obvious, although it would require illustrations of dissected skulls for a satisfactory comparison. A striking point of agreement in the early development of birds and crocodiles, and difference from other reptiles, lies in the formation and backward growth of infrapolar processes¹²; if it is assumed that the rear parts

of the basiptyergoid processes grew backward in avian evolution in similar fashion to those of crocodiles, then it seems highly likely that they are represented (in a very much flattened form) by the paired basitemporals of the bird skull. The latter thus probably originated from replacement bones, as Müller¹¹ has shown for the crocodilian "basitemporals". It is rather curious that the evolution of the modern crocodilian skull was accompanied by strong antero-posterior compression of the basicranium, whereas in the case of birds the basicranium became flattened and the carotoids were spread apart, so that pocr and sbr of Fig. 1b became confluent between them. Nevertheless, these two widely divergent types can be derived from the *Sphenosuchus* pattern without too much difficulty.

The region of the inner ear is well preserved on both sides of the fossil, and it has been possible to remove the matrix completely from the vestibule and cochlear recess; on the left side a fracture has exposed a vertical section through the capsule. The positions of the sutures and foramina are thus as well known as in a modern reptile

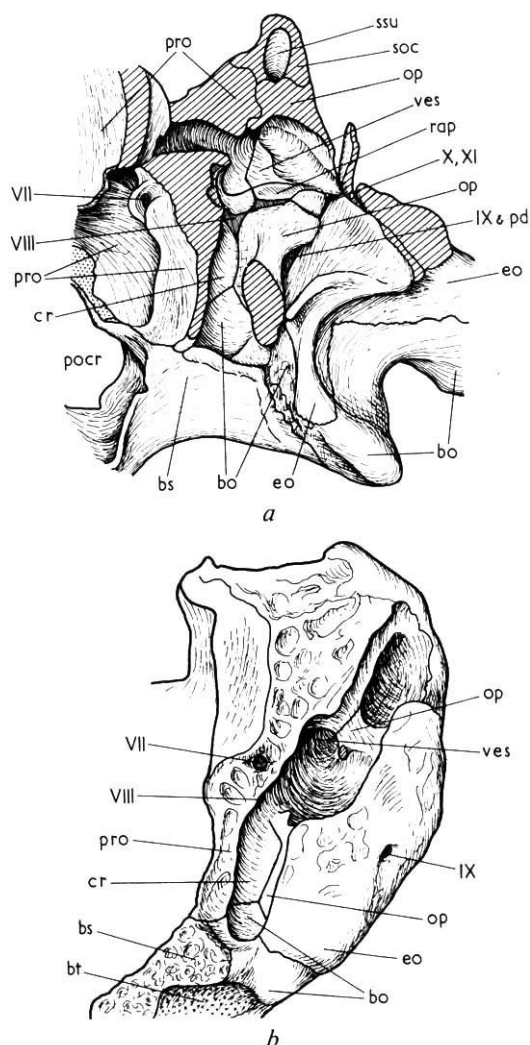


Fig. 6 Sutures in the cochlear recess. In each case the basi-sphenoid laps against the lagenar region, which is largely formed by the basioccipital. a, Natural parasagittal section through left inner ear region of *Sphenosuchus*. Slight disarticulation has occurred behind the cochlear nerve foramen. Broken bone cross-hatched ($\times 2.5$); b, dissection of rear part of cranium of young partridge (*Perdix perdix*), seen obliquely from below left and in front. A thin "skin" of basisphenoid and basitemporal has been left on the anterior and ventral surfaces of the basioccipital ($\times 7$); cr, cochlear recess; pd, perilymphatic duct; rap, groove for ramulus ampullae posterioris of cochlear nerve; ssu, sinus superior utriculi; ves, vestibule; VIII, cochlear branch of auditory nerve.

skull. The structures prove to be extremely similar, even in detail, to those in living crocodiles (Fig. 1b, d), in which, however, the tympanic cavity is almost completely enclosed by bone which surrounds the issuing nerves and the internal carotid artery. It is of particular interest that the cochlear recess is an elongated tubular structure. This housed the actual organ of hearing, indicating good ability in this respect. But comparison of the "simple crocodilian" structure of the *Sphenosuchus* ear with that of a young bird in which the sutures are still discernible, for example, a partridge (*Perdix perdix*), reveals an even closer resemblance in several respects than with the crocodile (Fig. 6). Thus the vestibule is in line with the long axis of the cochlear recess rather than lying behind it, and the basioccipital takes a greater share at the lower end in the cup housing the lagena. The elongated cochlear structures of birds and crocodiles, which resemble each other very closely and are more advanced than in other reptiles¹³, are usually assumed to be independently derived from thecodonts and to have been common to all archosaurs¹⁴. A similar structure cannot, however, be inferred for any thecodont skull so far described, and the shortness (in the vertical sense) in this group of that part of the prootic adjacent to the fenestra ovalis (Fig. 1a) strongly suggests that it did not occur^{3,15-17}. The development of an elongated cochlear duct seems rather to be part of a general improvement in hearing structures which, because of the forward and inward movement of the quadrate head, also involved the formation of a spacious tympanic cavity protected behind by the paracipital process, and of a definite external auditory meatus that is lacking in most reptiles. There is no reason to suppose that this group of associated changes occurred more than once in archosaurian history. According to Langston¹⁸, a somewhat elongated lagena (that is, cochlea) is a common dinosaurian feature, but the tubular cochlear recess and characteristic build of the otic capsule of crocodiles and birds does not seem to occur in dinosaurs.

Common Origin

The evidence from the nature of the quadrate articulation, the inner ear region, the homologies of the pneumatic spaces connected with the middle ear, the vestiges of kinetism, the palatal structure, the occiput and the peculiar basiptyergoid processes of *Sphenosuchus* together suggest a common origin for birds and crocodiles. This is in agreement with embryological resemblances emphasized by Müller¹¹. The clear conclusion emerges, however, that many supposedly diagnostic crocodilian characters are consequences of the fixation of a former streptostylic-kinetic system. The most important of these have already been mentioned. Thus the indications are that the skull of *Sphenosuchus*, although technically crocodilian, was derived from a type with a primitive avian kinetic system and many other bird-like features. Although it occurred too late itself to be an actual ancestor of most crocodiles, *Sphenosuchus* nevertheless probably corresponds fairly closely to an early stage in the evolution of the group. The recently proposed⁴ classification of crocodiles now seems to have been oversimplified and it would seem more likely that, on the whole, cursorial forms preceded and gave rise to amphibious types—as Kermack¹⁹ suggested—probably because of competition from bipedal coelurosaurs (small agile dinosaurs which arose in the late Trias). The difference in position of the vestibule relative to the cochlea in modern crocodiles compared with that of *Sphenosuchus* is in keeping with a trend towards flattening of the skull, and there has been a significant shift in emphasis in the pneumatic spaces from a low to a high position, the latter giving greater stability to the head of an amphibious animal.

It thus seems logical to consider seriously the possibility that crocodiles as a whole may have descended, perhaps as

successive "waves", from an unknown stock of late Middle to Upper Triassic reptiles which eventually gave rise to birds, and which may for convenience be called "proavians". Although the postulate of a common origin for birds and crocodiles does not necessarily imply an arboreal origin for the latter, the high degree of pneumatization of the skull of *Sphenosuchus* and other Triassic crocodilomorphs, and the slender, hollow bones of the limbs and girdles, suggests that this is a distinct possibility. Examination of the anatomy of living and fossil crocodiles lends further support to this view.

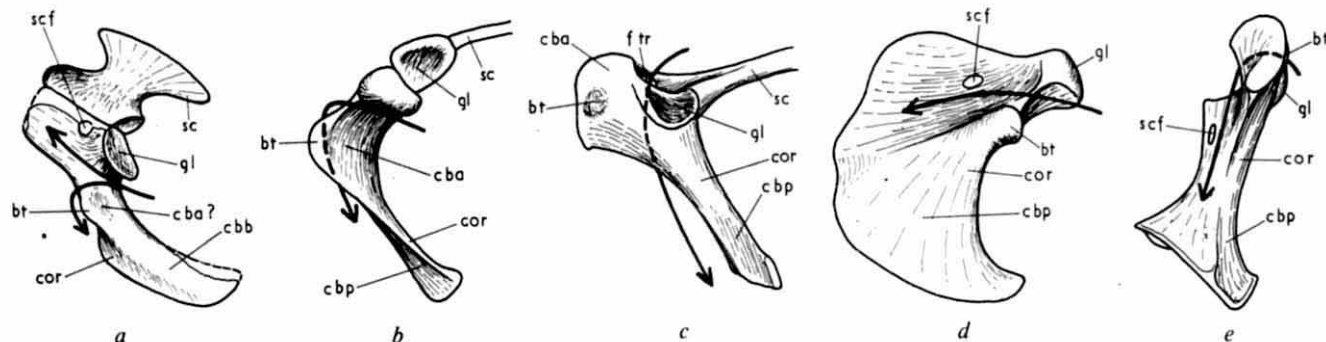


Fig. 7 The evolution of the bird coracoid. *a*, Ventrolateral view of left coracoid and scapula of *Sphenosuchus* (scapula foreshortened) ($\times 0.46$); *b*, *d*, lateral and anteroventrolateral views of left coracoid and part of scapula of *Archaeopteryx* (BM specimen) ($\times 1.84$); *c*, *e*, lateral and anteroventrolateral views of left coracoid and part of scapula of sea eagle, *Haliaeetus albicilla* ($\times 0.46$). —, Generalized line of action of the supracoracoideus. bt, Biceps tubercle; cba, surface for coracobrachialis anterior; cbb, coracobrachialis brevis; cbp, coracobrachialis posterior; cor, coracoid; f tr, foramen triosseum; gl, glenoid; sc, scapula; scf, supracoracoid foramen.

It has been suggested that the tendency to elongate the coracoid in early crocodilians, and the loss of the clavicle, were connected with cursorial galloping habits⁴, and an analogy was in fact drawn with the flapping flight of birds, which also have elongated coracoids. It is, however, possible to see these changes, and particularly the formation of a biceps tubercle (Fig. 7a) in *Sphenosuchus* and its Triassic allies, as arboreal adaptations in the first instance, which led on to the unique avian condition of the shoulder-girdle, but which also proved preadaptive (but probably only during early stages) for cursorial activity in forms reverting to a ground life. The clavicle is still present in *Sphenosuchus*, but apparently had only a ligamentous connexion with the interclavicle—a very bird-like feature. In the typical bird shoulder-girdle (Fig. 7c, e) the supracoracoideus muscle passes as a tendon through the foramen triosseum between scapula, coracoid and clavicle, and, inserting near the head of the humerus, acts as the most efficient elevator of the wing. The evolution of this pulley system can be traced from *Sphenosuchus* through *Archaeopteryx*²⁰, with forward and upward migration of the tubercle until it eventually contacted the clavicle, and inward rotation of the originally anterior part of the coracoid and scapula. The advantage of this arrangement was apparently that it permitted elongation of the supracoracoideus and at the same time ensured that the latter exerted a forward pull in a constant direction. In earlier stages, however, the supracoracoideus could only have functioned as a protractor, presumably bringing the forelimb quickly forward through a wide arc in climbing and jumping, and later, gliding. In *Archaeopteryx* (Fig. 7b, d) the biceps tubercle is still well below the glenoid, there was no foramen triosseum, and hence the supracoracoideus could not have assisted directly in flapping flight. Furthermore, recent examination of the "sternum" of the specimen in the British Museum leads to the conclusion that this actually consists of parts of four cervical vertebrae. Thus, as well as lacking an efficient wing elevator, *Archaeopteryx* probably had only a rather narrow, cartilaginous sternum, as there is no reason to suppose that the coracoids were widely separated. These points

have an obvious bearing on the recent discussion concerning the flying ability of *Archaeopteryx*²¹⁻²⁴.

The forearm and hand of *Sphenosuchus* are unknown, but there is every reason to think, from the evidence of allied Triassic forms^{4,19,25,26}, that the radiale and ulnare were differentially elongated in the distinctive crocodilian manner (Fig. 8a). It is not generally recognised that functionally and anatomically, the bird and crocodilian elbow and wrist joints are surprisingly similar. Essentially, an oblique ridge on the humerus pushes the radius distally when the elbow is flexed, causing the radiale, ulnare and hand to rotate laterally

around the distal end of the ulna. This is also the basic mechanism for folding the bird wing (Fig. 8b). Its significance in crocodiles is not entirely clear, but it may have arisen originally as a grasping adaptation during climbing and jumping—in early life crocodiles are active climbers²⁷—giving an extra joint and enabling the hand to extend further round cylindrical objects such as branches and tree-trunks. In bird and crocodilian embryos also there is a striking similarity in the lateral deflexion of the wrist-hand axis from that of the forearm, and in the reduction of the two outer digits²⁸.

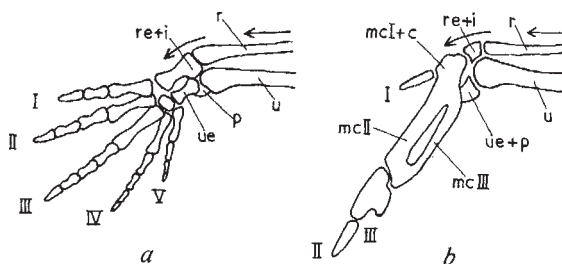


Fig. 8 Diagrams of the left wrist and hand in *a*, *Alligator*, and *b*, a bird from Gray³⁸. r, Radius; u, ulna; re, radiale; i, intermediate; ue, ulnare; p, pisiform; c, distal carpals; mc I-III, metacarpals I-III; I-V, digits I-V.

In both birds and ornithischian dinosaurs the pubis has rotated backward to lie alongside the ischium as a slender rod. According to Galton^{29,30}, this became possible when protraction of the hind limb was chiefly carried out by muscles originating from a long anterior process from the blade of the ilium, rather than from the pubis itself. Galton assumes, however, that the puboischiofemoralis internus 2 (in Romer's sense³¹) was a retractor of the femur, whereas it seems to me more likely that no muscle inserted on the summit of the greater trochanter and that pif2 was always a strong protractor in dinosaurs—a view which has been reached independently by A. Charig (personal communica-

tion). The only other comment that need be made here is that at least some Triassic crocodiles also had a well developed anterior iliac process³². The previously unexplained fact that, during the later part of the Trias, the crocodilian pubis became more slender and was gradually excluded from the acetabulum, now suggests some connexion with the proximal reduction and backward rotation of the bird pubis, which had already been accomplished in the earliest known bird, the Upper Jurassic *Archaeopteryx*. It is suggested that the crocodilian pubis became reduced in importance and "nearly turned back" during an arboreal stage, but was arrested because of a change in habits and was subsequently excluded from the acetabulum as a response to amphibious life. Apparently one crocodilomorph, namely the Upper Jurassic (possibly Upper Triassic) *Hallopus*⁴ (Fig. 9), did succeed in turning the pubis back—at any rate it is hard to see bone 24 of my diagram, considered but rejected as a pubis, in any other light, especially in view of the bird-like femur of this animal.

The hypothesis of an arboreal origin for crocodiles by repeated regressions from a primitive proavian stock also involves the derivation of the bird ankle-joint from the crocodilian type, as the latter was already well established in Triassic members of the group^{5,19,32-34}, and was evidently also present in *Sphenosuchus*. The essence of the change is believed to lie in the conversion of the crocodilian calcaneal heel, whose lever action would have been important in climbing and jumping, to the bird tibial cartilage as part of a "tendon-sling" tarsus that is more effective when landing on the hind limbs alone. The calcaneal heel and tibial cartilage have similar relationships to the gastroc-

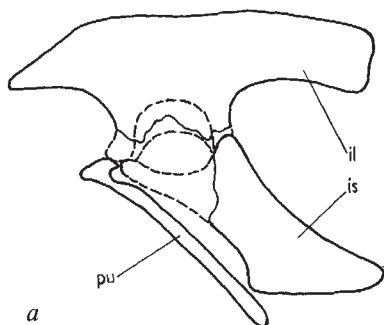


Fig. 9 New restoration of the pelvis of *Hallopus victor* (Marsh), in left lateral view ($\times 3/4$), pubis and ischium foreshortened, the pubis from a colour transparency. il, Ilium; is, ischium; pu, pubis.

nemius and peroneus longus³⁵ and each is functionally part of the foot, with which it rotates; in the crocodile the heel ossifies from a separate centre²⁸. During the evolution of the bird tarsus transverse compression seems to have caused the calcaneal heel to move medially, losing ossification, and its medially-facing sinus has apparently engulfed the flexor tendons which, in the bird, pass in canals through the tibial cartilage. Freed from its connexion with the heel, the body of the calcaneum would no longer be obliged to rotate with the foot and could transfer its allegiance to the crus, thus perfecting the mesotarsal joint. In the bird embryo the tarsus has a distinctly crocodilian appearance³⁶, with a dorsolateral process of the astragalus contacting the lower end of the fibula above the calcaneum. It is difficult to see why the anteriorly placed ascending process in the bird should develop in this lateral position if it is simply a newly evolved stabilizing structure. It is suggested that the crocodilian dorsolateral process (which lies partly behind the tibia) has migrated forward, because the forces tending to rotate the astragalus in quadrupedal locomotion are greatest

when the cruro-metatarsal angle is opening (during the propulsive phase), but in bipedal locomotion these forces are greatest when this angle is closing (during landing). The ascending process presumably takes up a position best suited to resist rotation. The tarsus of prosauropod dinosaurs illustrates an intermediate stage of this migration.

Other indications of arboreal adaptation in *Sphenosuchus* include the relatively long humerus, the marked inward and forward curvature of the lower half of the tibia, and reduction of the first metatarsal.

From the wealth of detailed evidence in the skull of *Sphenosuchus*, and from embryological and other resemblances between birds and crocodiles, it is difficult to avoid the conclusion that these two groups are much more closely related than has hitherto been conceded. As a result, it can no longer be assumed that anatomical structures which they have in common were also present in Triassic thecodonts, still less in dinosaurs or flying reptiles. Living crocodiles seem, to a surprising extent, to represent a "frozen" stage in the evolution of birds, and their Triassic ancestors seem to have been "drop outs" from a process of clandestine evolution which remains largely unknown to us.

I intend to give full acknowledgment to the many people who have helped me in this study in a detailed monograph on *Sphenosuchus* which is in preparation. At this time I would particularly thank Mr David Burleigh, of the Hancock Museum, Newcastle upon Tyne, for the loan of ornithological material.

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Further Evidence of Lower Pleistocene Hominids from East Rudolf, North Kenya, 1971

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In 1971 more early hominid material was recovered from the East Rudolf area and the total number of specimens now exceeds forty-five. The collection confirms the contemporaneity of *Australopithecus* and *Homo* during the Lower Pleistocene in East Africa.

LAST year I gave a preliminary report¹ of a collection of Lower Pleistocene hominids recovered from the East Rudolf area during 1970, which brought the total number of hominid

Table 1 Specimens Attributed to *Australopithecus* in the 1971 Collection from East Rudolf

Locality	Museum accession No.	Details of specimens
Ileret	KNM-ER 801 *	Right mandibular corpus—M ₂ , M ₃ in place and isolated right I ₂ , left M ₂ and M ₃
Ileret	KNM-ER 802 *	Isolated adult teeth—lower left P ₄ , M ₁ , M ₂ , M ₃ , and right P ₄ , M ₁ , M ₃ ; upper left C, P ³ and right M ²
Ileret	KNM-ER 805	Left mandibular corpus—lacking dentition
Koobi Fora	KNM-ER 810	Left mandibular corpus through symphysis—M ₃
Koobi Fora	KNM-ER 812	Immature left mandibular corpus
Koobi Fora	KNM-ER 814	Cranial fragments—including frontal and parietal
Ileret	KNM-ER 815	Left femur—proximal end lacking head
Koobi Fora	KNM-ER 816	Isolated left upper canine and fragments of molars
Ileret	KNM-ER 818	Left mandibular corpus—P ₄ to M ₃
Ileret	KNM-ER 819	Left mandibular corpus—lacking dentition
Ileret	KNM-ER 993	Right femur—diaphysis and distal epiphysis
Koobi Fora	KNM-ER 997	Proximal metatarsal
Koobi Fora	KNM-ER 996	Isolated lateral incisor
Ileret	KNM-ER 1170 *	Skull fragments
Ileret	KNM-ER 1171 *	Isolated juvenile teeth—lower left P ₄ , M ₁ , M ₂ , and right M ₁ , M ₂ ; upper left M ¹ , M ² and right C, P ⁴ , M ²

* These specimens are from one site. This material will be described in full elsewhere.

Table 2 Specimens Attributed to *Homo* in the 1971 Collection from East Rudolf

Locality	Museum accession No.	Details of specimens
Koobi Fora	KNM-ER 164 *	Two metacarpus and two cervical vertebrae
Ileret	KNM-ER 803	Upper canine, medial incisor and associated postcranial parts—including fragments of the diaphyses of femur, tibia, ulna and fibula, fragments of distal fibula, talus, second metatarsal and several phalanges
Ileret	KNM-ER 806	Isolated teeth—M ₁ , M ₂ , two P ₄ s and two M ₃ s
Ileret	KNM-ER 807	Fragment left maxilla—M ³ and partial M ²
Ileret	KNM-ER 808	Isolated upper teeth—lateral incisor, premolar, and M ¹
Ileret	KNM-ER 809	Fragments isolated teeth
Koobi Fora	KNM-ER 811	Fragment parietal
Koobi Fora	KNM-ER 813	Right talus and fragment tibia diaphysis
Koobi Fora	KNM-ER 817	Left mandibular corpus—lacking dentition
Ileret	KNM-ER 820	Immature mandible—permanent incisors and first molars and deciduous canines and molars. The unerupted permanent premolars and canines are in the crypts
Ileret	KNM-ER 992	Mandible—lacking three incisors and parts of the ascending rami

* Associated with the parietal fragment collected during 1969². This material will be described in full elsewhere.

specimens from East Rudolf to twenty-three, including both cranial and postcranial elements²⁻⁴. No attempt has yet been made to assign specific taxonomic nomenclature to this material.

In 1971 more hominid specimens were recovered from East Rudolf (see Tables 1 and 2), and the total number of hominid specimens from the area now exceeds forty-five. The exact location of each hominid find is marked in the field by an embedded concrete post and geological investigations have allowed all the specimens to be related to stratigraphic sections. Several excavations were undertaken to recover further material associated with surface finds.

Preliminary reports of geological investigations in 1969 and 1970 in the two principal collecting areas at East Rudolf—Koobi Fora and Ileret—have been published^{5,6}, but until 1971 no stratigraphic correlations between the two areas had been

obtained. Mr Bruce Bowen (Iowa State University) continued geological investigations in both areas in 1971 and in addition to obtaining more detailed stratigraphic correlations between the principal fossiliferous localities in each area, he was also able to obtain a probable stratigraphic correlation between the two areas (see Fig. 1).

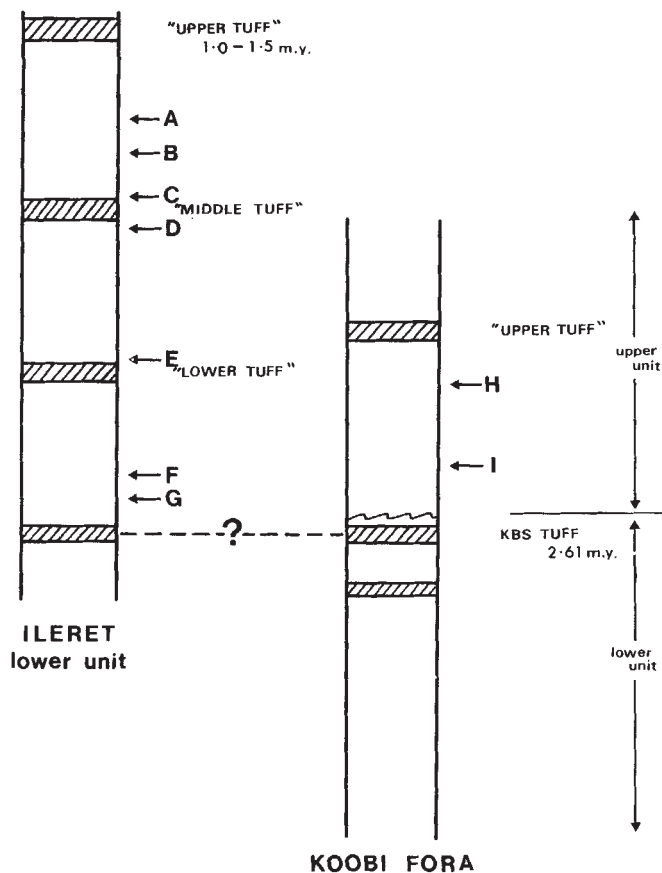


Fig. 1 Generalized stratigraphic sections of the lower unit of the Ileret sedimentary sequence (left) and the Koobi Fora sedimentary sequence (right) to show their probable stratigraphic correlations and the stratigraphic position of the hominid specimens collected between 1968 and 1971. Specimens attributable to *Homo* are marked by an asterisk in the following key: A=KNM-ER 725, 726, 727, 728, 731*, 739, 740, 741, 801, 802, 805, 818, 819, 992*, 993, 1170, 1171; B=KNM-ER 404; C=KNM-ER 729; D=KNM-ER 733, 803*, 806*, 807*, 808*, 809*, 820*; E=KNM-ER 727; F=KNM-ER 406, 732, 815; G=KNM-ER 407; H=KNM-ER 403, 730*, 734*, 736, 737*; I=KNM-ER 164*, 405, 738, 810, 811*, 812, 813*, 814, 816, 997, 998.

Although the Ileret sequence shows no major unconformities there seems to be a localized unconformity in the Koobi Fora succession. The chronological significance of this feature is not yet fully clear but it is unlikely to be of great importance. At the time of writing this report, there were no radiometric dates available for the tuffaceous horizons in the Ileret sequence but a date of greater than 1 m.y. for the "Upper Tuff" Ileret is considered probable on the basis of faunal evidence.

It should be noted here that the stratigraphic position of the two crania (KNM-ER 406 (FS-158) and KNM-ER 407 (FS-210)) reported earlier by me² is probably incorrect, because it now seems that both these specimens are from a horizon that is most likely above rather than below the tuff dated at 2.61 m.y.⁷. The exact age of the two specimens is therefore unknown, but at present faunal and other evidence suggests a date of less than 2.6 m.y. Similarly, all the other hominid

material recovered so far from East Rudolf, with the possible exception of two specimens, KNM-ER 417 and KNM-ER 817, seems to be younger than 2.61 m.y.

Collection of samples for radiometric dating was undertaken during 1971 by Mr Ian Findlater (Birkbeck College, London), and Dr A. Brock (University of Nairobi) is currently carrying out systematic palaeomagnetic investigations to complement this dating programme. More precise information on the age of the hominid specimens should thus be available soon, but until these results are known, the hominid material should be considered as being from a range of deposits aged from approximately 2.5 m.y. to 1.0 m.y.

A team led by Dr V. J. Maglio (Harvard University) investigated the faunal evidence from East Rudolf in 1971 and collected samples of fossil vertebrates from many of the more important localities. This collection will be discussed elsewhere. It should, of course, provide a further indication of the age of the fossiliferous localities.



Fig. 2 Mandible KNM-ER 818; above lateral view, below occlusal view.

A large collection of artefacts was recovered in 1971 *in situ* by Professor Glynn Isaac (University of California, Berkeley) and are described elsewhere⁸. Several new artefact localities were discovered and these will considerably broaden the evidence of early Pleistocene palaeotechnology. Although some of these newly located sites occur in the same 2.61 m.y. horizon that has previously yielded artefacts, a number seem to fall above this horizon.

The 1971 collection includes cranial and postcranial elements of both *Homo* and *Australopithecus* (as defined by Tobias⁹) and thus confirms the contemporaneity of two hominid genera, *Homo* and *Australopithecus*¹. The increased sample of *Australopithecus* specimens (see Table 1) suggests a wide range of individual size variability within one species, and the additional postcranial specimens support other evidence that the Lower Pleistocene examples of *Australopithecus* from East Africa are distinct from *Homo* in limb morphology as well as in cranial morphology.

Australopithecine Material

The cranial and mandibular specimens of *Australopithecus* are very similar to previously described material from East Rudolf and are probably further examples of the same species. The mandibular specimen, KNM-ER 818, is worth special mention in view of its immense size (see Table 3 and Fig. 2). When compared only with the small mandibular specimens in the East Rudolf collection, this mandible seems too large to be placed within the same species as these specimens, but when considered with the entire sample (Fig. 3) the inclusion within one species is acceptable.

The immature mandibular fragment, KNM-ER 812, should yield valuable data when studied in detail. The outer surface of the specimen has been abraded with loss of detail but this has resulted in the exposure of several permanent teeth within their crypts. A comprehensive description will be published elsewhere following X-rays and careful preparation of the specimen.

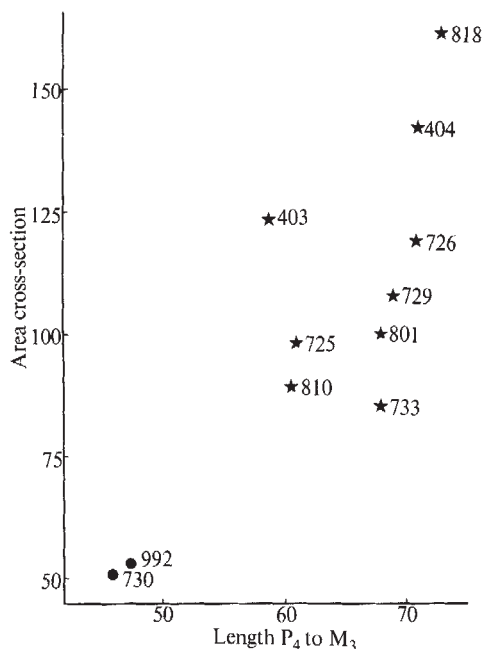


Fig. 3 Graphical illustration to show both the mandibular variability among the specimens of *Australopithecus* (★) from East Rudolf and their clear separation from the specimens of *Homo* (●). The estimated area of cross-section of the mandibular corpus in the region of M₂ (which was considered as an ellipse and was obtained by multiplying π by half the maximum width by half the depth in this region) is plotted against the distance between the anterior roots of P₄ and the posterior roots of M₃ measured at the cervix. Where slight damage to the mandible prevented these dimensions being taken accurately, estimated measurements have been used. The KNM-ER number of the specimen is given beside each point.

The two femoral fragments, KNM-ER 815 and 993, are interesting. The fragment of the proximal region, KNM-ER 815, shows marked antero-posterior compression of both the neck and the shaft distal to the lesser trochanter, as has been observed in other specimens, particularly the similar fragment

from Olduvai (Old. Hom. 20)¹⁰ and this character should be important to a functional interpretation of the postcranial elements of *Australopithecus*. The right femur, KNM-ER 993, consists of more than two-thirds of the shaft with the distal region attached (see Fig. 4). Both condyles are weathered but the femoro-condylar angle and various other features are preserved.

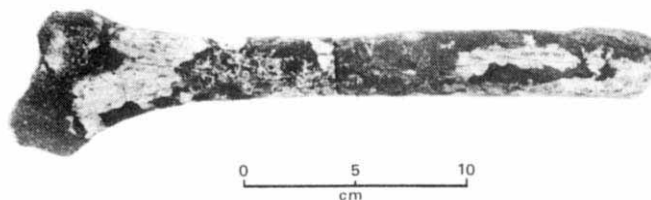


Fig. 4 Anterior view of femur KNM-ER 993.

Homo Material

In 1970 three specimens were collected which were distinctive from the other hominid specimens from the East Rudolf area and were assigned to the genus *Homo*¹. Eleven more *Homo* specimens were collected in 1971 (see Table 2), and five of these deserve special mention here.



Fig. 5 Occlusal view of mandible KNM-ER 820.

The juvenile mandible, KNM-ER 820 (see Fig. 5 and Table 4), was collected from a horizon in the Illet area below the "middle tuff". The left and right deciduous molars have not yet been shed and the unerrupted premolars and canines are within their crypts. The relative size of the incisors to the first molars, the form of the mandibular body and arch, and the detailed morphology of the dentition all demonstrate the distinctiveness of this mandible when compared with mandibular remains of *Australopithecus* from both the same stratigraphic horizon and elsewhere.

Table 4 Measurements (in mm) on the Dentition of the Mandible KNM-ER 820

Tooth	Left		Right	
	Mesiodistal	Buccolingual	Mesiodistal	Buccolingual
I ₁	6.2	6.2	6.2	6.3
I ₂	7.5	7.0	7.5	6.7
dm ₁	—	7.8	8.9	7.8
dm ₂	11.4	9.5	11.5	9.5
M ₁	12.0	10.8	12.0	10.9

The adult mandible, KNM-ER 992 (see Fig. 6 and Table 5), was recovered from a horizon in the Illet area which has yielded several specimens of *Australopithecus*. It was taken

Table 3 Measurements (in mm) on the Mandible KNM-ER 818 (Parentheses indicate Estimated Measurements)

Height of mandibular body at M ₂	(54)
Height of mandibular body at P ₄	(55)
Breadth of mandibular body below M ₂	(38)
Length of molar-premolar series	(87)

from a horizon above the "middle tuff" and is thus slightly more recent than KNM-ER 820.

The mandibular fragment, KNM-ER 817, was collected from a locality south of Koobi Fora, Area 123, the geology of which has not yet been investigated in detail, but the faunal

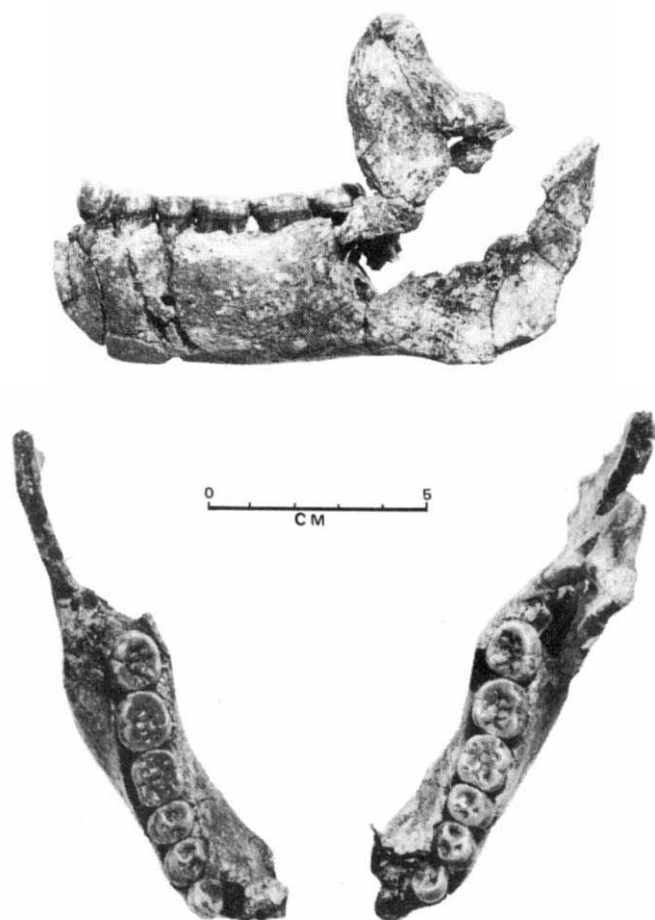


Fig. 6 Mandible KNM-ER 992; above lateral view, below occlusal view.

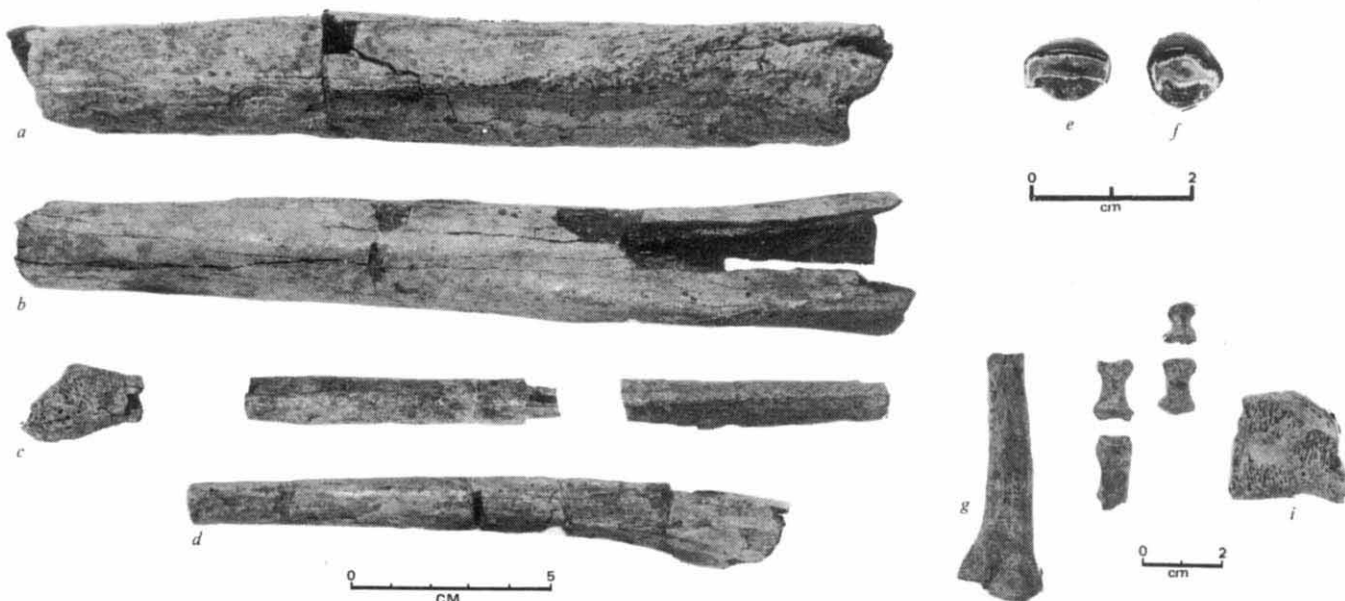


Fig. 7 Specimen KNM-ER 803. *a*, Posterior view of left femur; *b*, lateral view of left tibia; *c*, fragments of fibula; *d*, medial view of left ulna; *e*, upper medial incisor; *f*, upper canine; *g*, lateral view of left third metatarsal; *h*, phalanges; *i*, lateral view of fragment of left talus.

Table 5 Measurements (in mm) on (*a*) the Mandible and (*b*) the Dentition of KNM-ER 992 (Parentheses indicate Estimated Measurements)

(a)	Left	Right		
Height of mandibular body at M ₂	(32.6)	(33.4)		
Height of mandibular body at P ₄	(32)	(30)		
Breadth of mandibular body below M ₂	20.5	23		
Length of molar-premolar series	56.5	56.5		
Height of ascending ramus (measured to coronoid process)	(78)	—		
(b)				
Tooth	Left		Right	
	Mesiodistal	Buccolingual	Mesiodistal	Buccolingual
I ₂	7.3	7.2	—	—
C	9.0	—	9.0	9.2
P ₃	9.5	11.0	9.5	11.0
P ₄	8.9	11.0	8.6	11.0
M ₁	12.5	11.0	12.4	10.5
M ₂	12.8	12.0	13.4	12.0
M ₃	12.8	12.0	13.0	12.2

elements suggest an earlier period than is represented at both Ileret and the upper fossiliferous deposits at Koobi Fora. The gracility and general morphology of the mandibular corpus of this specimen, together with the proportions of the dentition, as indicated by the roots and root sockets, suggest an affinity with *Homo* rather than *Australopithecus*, but more specimens are needed to establish this point. The stratigraphic position of the site also needs to be investigated further.

The partial, fragmented skeleton, KNM-ER 803 (see Fig. 7), was collected by my wife, Dr Meave Leakey. The two teeth, an upper canine and an upper lateral incisor, are probably sufficient for positive generic identification but the associated postcranial elements are also quite distinctive. A preliminary excavation was made to recover further parts but without success; this work will be extended in the next field season. The specimen is to be studied and described in detail by Dr M. H. Day.

The taxonomic identification of the talus and tibia fragment, KNM-ER 813 (see Fig. 8), cannot be certain but the morphology of the talus is similar in many respects to modern man and

seems to have been well suited to a fully bipedal form of locomotion (personal communication from M. H. Day and B. A. Wood).

Significance of the 1971 Expedition

The 1971 collection confirms earlier evidence that there is only one species of *Australopithecus* at East Rudolf. Within this species there is considerable individual variation in size and I consider that this can be explained in part by sexual dimorphism. Fig. 2 shows the range of the mandibular material and although this variation is greater than can be expected in a contemporary population of modern man, some extant primates such as *Papio* and *Gorilla* show wide variation between individuals. In view of the variability of the East Rudolf *Australopithecus* the question of species identification will be left open until further and more complete material is available. At present, there is evidence to suggest that specimens of *Australopithecus* from East Rudolf are very similar to representatives of the genus from Olduvai and the Omo Valley and it is possible to speculate that in the Lower Pleistocene there was an East African form of *Australopithecus* which was morphologically distinctive from a contemporary form living in southern Africa. In my view there is no clear evidence for two contemporaneous species of *Australopithecus* in East Africa in the Lower Pleistocene.



Fig. 8 Superior view of right talus KNM-ER 813.

There now seems little doubt that there are some specimens attributable to *Homo* from East Rudolf; these have been recovered from various localities and horizons, and in each case, the same horizons have also yielded specimens attributable to *Australopithecus* (see Fig. 1). The coexistence of two hominid lineages at East Rudolf during the Lower Pleistocene is thus confirmed and examination of Lower Pleistocene hominid collections from Olduvai and Omo suggest a similar conclusion.

A striking feature of the *Australopithecus* lineage, when compared with that of *Homo*, is seen in the dentition—the posterior teeth are considerably larger relative to the anterior teeth in *Australopithecus* than in *Homo*. This is best shown in the dentition of the two most complete mandibular specimens from East Rudolf, KNM-ER 729 (*Australopithecus*) and KNM-ER 992 (*Homo*) and is illustrated in Fig. 9 where the length and breadth of the individual teeth have been multiplied

and plotted. The separation of *Homo* and *Australopithecus* lineages can also be supported on other morphological grounds such as the relative gracility of the *Homo* mandible (see Fig. 3).

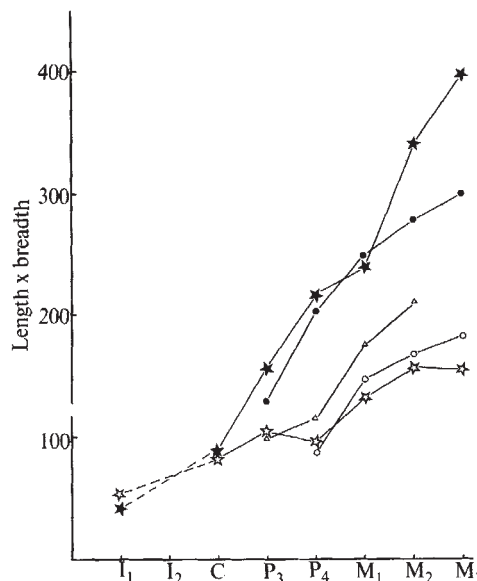


Fig. 9 Graphical illustration to show the larger posterior dentition relative to the anterior dentition in *Australopithecus* compared with the condition in *Homo*. The almost complete *Australopithecus* mandible from East Rudolf, KNM-ER 729 (★), is compared with the almost complete *Homo* mandible, KNM-ER 992 (☆). The mesiodistal dimension has been multiplied by the maximum buccolingual dimension for each tooth. Where both the left and right teeth are preserved the mean dimension has been used. Values for the posterior dentition of the following specimens have been calculated from published dimensions and are also included: ●, the *Australopithecus* mandible from Peninj (Tobias⁹); △, the type mandible of *Homo habilis*, (Old. Hom. 7) (Tobias¹¹); ○, the paratype of *Homo habilis*, (Old. Hom. 13) (Tobias and Koenigswald¹³).

The mandibular specimens from East Rudolf attributed to *Homo* are remarkably similar morphologically and in dental proportions to some specimens that are presently included in the *Australopithecus africanus* collection from Sterkfontein, South Africa (for example, Sts 52b). Does this imply that the East Rudolf specimens should be included within *A. africanus* as defined in the literature? I believe that the *A. africanus* collection includes specimens that represent two separate lineages—*Australopithecus* and *Homo*—and the new evidence from East Rudolf illustrates the importance of a re-examination of the earlier collections which serve as the basis for much of the current thought and teaching on human evolution.

The postcranial elements of the East Rudolf collection should provide a firmer basis for the interpretation of early hominid locomotion. Unfortunately there is only one specimen which shows a clear association of cranial and postcranial parts, and until further examples are obtained, taxonomic identifications must remain provisional. Nevertheless the East Rudolf material indicates two quite distinct forms on the basis of morphology rather than size and this evidence is thus consistent with the cranial evidence.

The isolated talus from Koobi Fora (KNM-ER 813) is of particular interest in view of the study on the fossil tali from Olduvai (Old. Hom. 8) and Kromdraai¹¹. The East Rudolf specimen was recovered from a stratigraphic level where the faunal elements indicate an age equivalent to or slightly earlier than lower Bed I Olduvai from which the Olduvai talus was recovered. The East Rudolf talus has not been studied in detail but preliminary examination shows it to be remarkably

similar to the tali of modern man and distinctive from the two fossil tali from Olduvai and Kromdraai. Canonical analysis¹¹ on dimensions considered of functional importance, separated these two fossil tali from both a group containing "known striding bipedal man" and a group containing "known pronograde quadrupedal apes". It was concluded that each fossil talus came from the foot of a biped, but one in which the striding bipedal gait of modern man had not yet been achieved. Casts of the new talus from East Rudolf have been examined in a very preliminary manner by Day and Wood, and they have expressed their agreement with my view that the specimen seems closer to modern man in several features, the same features that also make it distinctive from both the Olduvai and the Kromdraai tali.

I thank the Committee for Research and Exploration of the National Geographic Society, the William H. Donner Foundation, the L. S. B. Leakey Foundation and the Astor Foundation for supporting this successful expedition. In addition, several friends kindly made personal donations. I also thank the Government of Kenya and the Museum Trustees of Kenya,

and all the participants in the expedition, particularly my wife Meave who had responsibilities for supplies and camp needs in addition to her scientific work in the field and at the museum.

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Active Galaxies with Radio Trails in Clusters

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New radio data and a critical examination of the interacting galaxies hypothesis to explain complex radio sources in clusters lead us to reject a causal relation between sources in clusters. Instead, the "head-tail" radio galaxies are proposed to represent radio trails of galaxies, active in their own right, along trajectories through a dense intergalactic medium.

HERE we describe some of the data obtained recently with the Westerbork synthesis radio telescope (WSRT) in an observing programme aimed at radio sources in clusters. We examine the hypothesis that the energy of certain peculiarly extended sources in a cluster originates in another, more compact radio source, sometimes hundreds of kpc away, and present radio evidence bringing new features to light which have a bearing on these problems. This leads to an alternative interpretation which avoids the many difficulties besetting the "interacting

galaxies" hypothesis and yields information about a cluster's intergalactic gas density.

The Perseus, Coma and 3C129 Clusters

Ryle and Windram¹ investigated the Perseus cluster with the Cambridge One Mile Telescope at 408 and 1,407 MHz. They measured the brightness distribution of radio sources identified with NGC 1265 and IC 310, both roughly half a degree from the active Seyfert galaxy NGC 1275, and found that these weak radio galaxies have "head-tail" radio configurations, with the optical object at the front of the head and the tail pointing away from the direction of NGC 1275 (= 3C84A) to within about 15°. All three radio sources are enveloped by a large low surface brightness source (3C84B). Ryle and Windram concluded that the radio sources of NGC 1265 and IC 310, weaker than 3C84A by factors of $\times 3$ and $\times 25$ respectively, are formed by energy transported from the active galaxy. In particular they chose a model where a relativistic particle stream, emitted from NGC 1275 over a large solid angle, interacts with the gaseous component of the galaxies encountered. The magnetic field in the latter then ensures both the dynamic coupling and the nonthermal radio emission.

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Willson² studied the Coma cluster and showed that it contains a pair of radio sources whose radio morphology, relative position and orientation are somewhat similar to the source pair associated with NGC 1275 and 1265. In this case the more compact source, 5C4.85, is four times weaker than the extended source 5C4.81. The tail points about 45° away from the line joining compact and extended source, in conflict with the simple model's prediction. Willson showed that this may well be explained by the relative motion of the galaxies during a few times 10⁷ yr, his estimate of the radio source lifetime.



3C1291 and 3C129

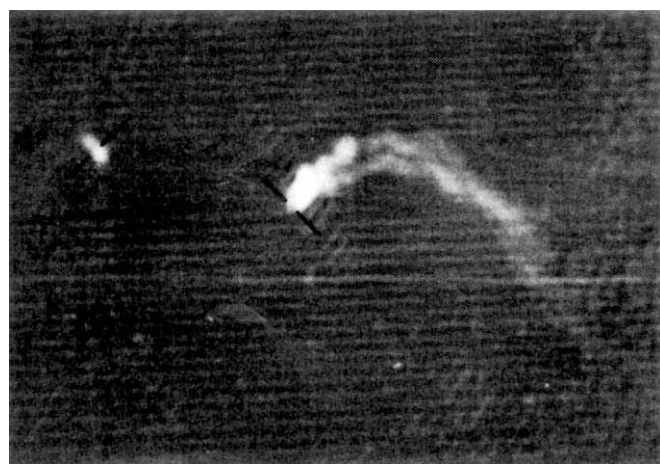
Fig. 1 1,415 MHz radiophotograph of 3C129 and 3C129.1. The effective resolution is 22×31 arc s; the sensitivity per synthesized beam ($5 \times \text{r.m.s.}$) is $2 \times 10^{-29} \text{ W m}^{-2} \text{ Hz}^{-1}$. The central depression results from the missing zero and 18 m baselines. Bars indicate the positions of the optical galaxies.

The radio source pair 3C129 and 129.1 has been mapped with the One Mile Telescope³, but it lies in an obscured region of the galactic plane and only recently have both members been convincingly identified⁴. Although spectroscopic information is lacking, the estimate based on corrected apparent magnitudes gives a distance comparable with the Perseus and Coma clusters (~ 50 Mpc). The pair has a radio configuration similar to those discussed above, and Hill and Longair⁴ conclude on the basis of a complete set of about 200 3C source maps that these three systems form a separate category in that set. All three are located in rich clusters and are intrinsically weak radio galaxies. Their space density is about one third that of such clusters, a suggestive criterion for further searches with a sensitive high resolution radio telescope. Hill and Longair adopt the Ryle and Windram hypothesis of interacting galaxies, assuming the compact radio source to be the origin of the extended source's energy. Unlike Willson, however, they do not follow the Ryle and Windram model of relativistic particle streams through large solid angles; instead they postulate highly collimated cold plasma streams from the compact to the extended radio source.

Interacting Galaxies Hypothesis

Several problems associated with the model of Ryle and Windram remain to be explained. For example, there are large spectral differences between the compact and extended sources; a past energy production rate in NGC 1275 much greater than its already large present power and the process of interaction between a stream of relativistic particles and a

galactic gaseous component remain to be explained. Several other galaxies in the Perseus cluster, closer to NGC 1275 than the two "head-tail" sources, are not detected as radio sources; this was attributed¹ to less significant gaseous components in these fainter systems which might make them somewhat weaker, and that is plausible in a region where detection is very difficult because of the proximity of the strong compact nuclear source in NGC 1275. For Willson's Coma cluster study this limitation is much more stringent because there the compact source which is the supposed origin of the particle streams is not nearly so dominant a radio source. Hill and Longair attempt to retain the interacting galaxies hypothesis but to accommodate several difficulties Willson already saw: the lack of radio emission from galaxies nearer to the compact source than the extended one and the much greater energy requirements of the compact than of the extended source (for the Coma and the 3C129 pairs the supposed source of all the energy is much the weakest radio emitter). Their arguments, used to account for the double structure of the



NGC 1265

Fig. 2 A 1,415 MHz radiophotograph of NGC 1265; resolution 22×33 arc s. Otherwise as Fig. 1.

heads in 3C129 and NGC 1265 and for the discrepancies of tail directions with respect to compact source locations, seem to us entirely *ad hoc*. The descriptive model and flow pattern illustrations invoke many poorly understood processes which add up to a physically improbable and unnecessarily complex system. Even if the cold plasma kinetic energy transport were sufficient, how that could produce the radio sources observed is quite obscure. Comparisons with jets such as that in M 87 or the bridges between the components of symmetric double radio galaxies lend no support to the collimated stream model of Hill and Longair, for, unlike their streams, these features are themselves detectable sources of nonthermal emission.

New Observations at 1,415 MHz

The WSRT is an east-west array of ten fixed and two movable 25 m paraboloids. Each dish has a dual polarization front end at its prime focus and each movable dish dipole is combined with each dipole of every fixed dish, to provide eighty interferometers, four for each of the twenty simultaneous baselines. A complete aperture synthesis consists of 4×12 h of data, giving eighty contiguous baselines from 36 to 1,458 m at 18 m intervals. These data are transformed into maps of

the four Stokes parameters, which have a tapered sensitivity from the single dish pattern with a 37 arc min diameter at the half-power points and whose definition is provided by the synthesized beam of $(22 \text{ arc s}) \times (22 \text{ arc s cosec } \delta)$. The r.m.s. noise at the field centre is $0.4 \times 10^{-29} \text{ W m}^{-2} \text{ Hz}^{-1}$ for full synthesis with the present receiver system (bandwidth 4 MHz, system temperature 240 K). For such an observation a perfect measurement gives a grating ring at 40 arc min in RA from a source.

The full baseline coverage and the gain and phase stability of the WSRT make it possible to map faint features near strong sources.

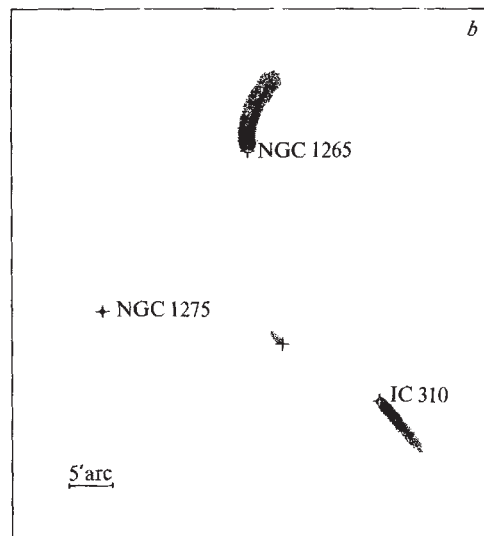
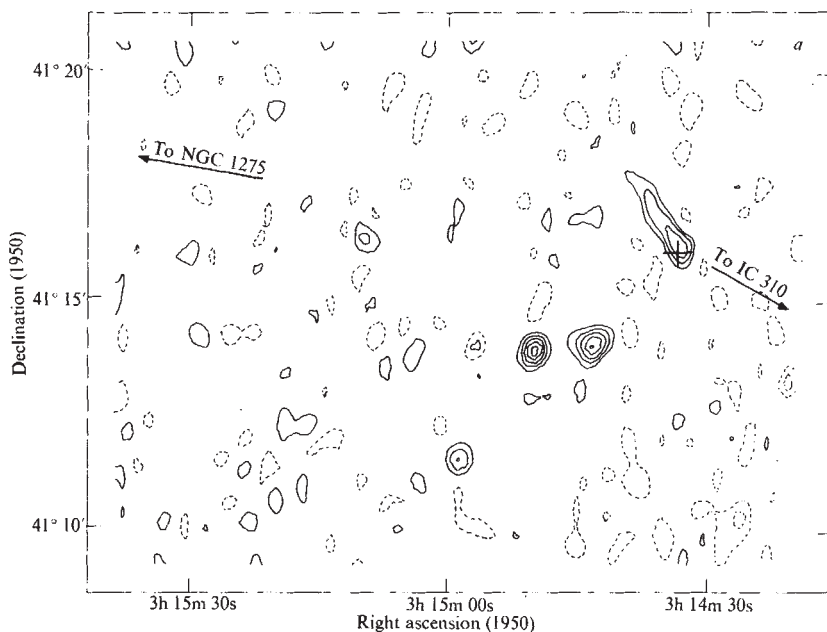


Fig. 3 *a*, A 15×11 arc min area from the full synthesis map centred on IC 310, showing the radio source associated with 15 of Chincarini and Rood⁵. Brightness contours are at 2.1 K intervals, excluding the zero level; dashed contours represent negative levels. The cross indicates the optical position. *b*, A schematic representation of NGC 1275 and the active galaxies with radio trails in the Perseus cluster, indicating relative positions and orientations. Crosses mark the optical positions.

Fig. 1 shows a 1,415 MHz full synthesis map of the 3C129/3C129.1 brightness distribution. The radiophotograph is a computer controlled cathode ray tube representation of the 512×512 intensity points in the map. We have found these radiophotographs useful complements to conventional contour maps. The head of 3C129 is double and the tail, not detected in the Cambridge 1,407 MHz map and not resolved at 408 MHz, is also clearly double. It has a length of more than 400 kpc and remains quite narrow. The linear polarization increases progressively along the tail from 5% near the head to more than 60% before it becomes noise limited. These data and their significance will be discussed in detail elsewhere. The structure of 3C129.1 is obscured by a grating ring in the Cambridge 1,407 MHz data; here it is clearly resolved. Its extent is about 2 arc min in a position angle of $\sim 45^\circ$. Its structure bears little resemblance to that of NGC 1275, the source presumed to have lit the extended sources in Perseus.

We made two complete synthesis maps of the Perseus cluster, one centred near NGC 1265 and one near IC 310. The former shows that the structure of NGC 1265 is similar to that of 3C129, with a double head and a tail which exhibits double structure and a high degree of polarization. Fig. 2 shows the radiophotograph. We suspect that much of the difference between Fig. 1 and Fig. 2 is a result of projection effects. The tail of 3C129 disappears into the noise, but the end of the

NGC 1265 tail is well defined. We presume that the latter lies in a plane which makes a small angle to the line of sight which may be tangential to the last part of the tail.

There is a chain of galaxies between NGC 1275 and IC 310. We detect three of them, one of which has a most interesting structure. It is the 15.4 mag. galaxy designated 15 by Chincarini and Rood⁵. Fig. 3*a* shows a contour map of the object and its surroundings, Fig. 3*b* is a schematic representation of the Seyfert galaxy, the three extended objects and their orientation. The newly detected galaxy has a 1,415 MHz flux density of $30 \pm 5 \times 10^{-29} \text{ W m}^{-2} \text{ Hz}^{-1}$ and its radio structure resembles that of NGC 1265 and IC 310. The region

of maximum brightness has the bright galaxy at its front edge and the pronounced radio tail points not away from but nearly towards NGC 1275.

Alternative Interpretation

The discovery of an object whose tail points in a direction counter to that predicted by the interacting galaxies theory indicates that the approximate alignment of both the NGC 1265 and IC 310 tails along radius vectors from NGC 1275 is fortuitous. In our view this is conclusive evidence that the "head-tail" radio galaxies are not caused by compact radio sources some hundreds of kiloparsecs away. We propose a simpler alternative explanation, namely that the extended sources are radio galaxies in their own right, causally independent of any other radio sources in their neighbourhood. The component distortion, the displacement of the optical galaxy to the front edge of the radio brightness distribution and the direction and form of the radio trail all suggest motion of a radio galaxy resisted through a gaseous medium.

What if the extended galaxies under discussion became radio sources by activity in their own nucleus? Apart from their tails, the radio structure of 3C129 and NGC 1265 is reminiscent of common double radio galaxies. According to widely accepted models of these sources, the radio emitting

material is ejected from the central region of a galaxy in two comparable, oppositely directed streams, which after emerging from the galaxy are prevented from expanding and dispersing by the dynamic pressure at the radio source boundary. This pressure arises from the inertia of the external medium encountered by the ejected material. We believe that the morphological peculiarities of some radio sources in clusters can be understood in the framework of these "ram pressure theories" (see refs. 6-8), provided the galaxy moves at very high speed through the cluster's intergalactic medium. Evidence that this is the case comes from the investigation of the Perseus cluster; Chincarini and Rood⁵ show that the radial velocity dispersion is $1,600 \text{ km s}^{-1}$, the largest known, and that NGC 1265 has radial velocity which differs from the cluster mean by $2,200 \text{ km s}^{-1}$.

The sources we have discussed are quite narrow along their entire length and the head components are elongated in the same direction. Model calculations indicate that the ejection velocity is at most of the order of the galaxy speed. In a WSRT investigation of complex 3C sources we have found a few cases, 3C465 among them, which can be interpreted as systems where the ejection speed is greater than the galaxy speed (Miley and Van der Laan, in preparation). These appear to be transitions from the well known doubles to the "head-tail" radio galaxies.

The slender curved extension suggests an orbit through the cluster along which a trail of radio emitting material has been left behind (Figs. 1 and 2). It is not, however, possible to estimate the cluster mass needed to produce the observed curvature because we lack knowledge of both the tangential velocities and the complete orbits. Careful inspection of the photograph of 3C129 suggests that the trail has the form of a double helix, which we speculate may reflect rotation of the region of the galaxy which is ejecting radio emitting material. Models for the formation, structure and confinement of the radio trails will be discussed elsewhere.

We note here that for a speed of $2,500 \text{ km s}^{-1}$, the 400 kpc trail of 3C129 implies an age of $2 \times 10^8 \text{ yr}$ for the end of the detectable feature. Radiative losses must have affected the spectrum of the trail⁹ so at high frequencies it will become undetectable at a smaller distance from the galaxy. Willson found such an effect for 5C4.81 in the Coma cluster. In addition to further high resolution 5 GHz studies we propose to search rich clusters for extended sources. One may expect to find some trails without detectable heads, because the former may persist longer than the galaxy's activity. Such searches are best carried out at low frequencies which are affected least by radiative losses and where the instrumental sensitivity to weak extended features is also much improved.

Density of the Intergalactic Medium

A striking feature of all five "head-tail" radio sources (three in the Perseus, one in the Coma, one in the 3C129 cluster) is that the optical galaxy lies at the front edge of the radio source. In our model the radio source originates in that galaxy but as soon as the radio emitting streams or clouds are exposed to the intergalactic medium their forward motion is resisted. The fact that the radio source front edge lags behind the galaxy implies that the mass density in the radio source is at most a few times the density of the surrounding medium and its velocity is quickly reduced as momentum is conserved. The second important implication of the galaxy's front edge position is that the dynamic pressure exceeds the internal pressure contributed by relativistic particles and magnetic fields in the radio components of the head: $\rho_{i.g.} v_{gal}^2 \gg p_{radio}$. If this inequality is not satisfied the radio source expands faster than v_{gal} and the radio source front edge will remain ahead of the galaxy. The minimum pressure can be calculated from the synchrotron emission theory for a source of known brightness and dimensions. The values are of the

same order in all cases considered here, $10^{-11} < p_{radio} < 1.4 \times 10^{-10} \text{ dyne cm}^{-2}$; the lower and upper estimate are for a relativistic proton contribution equal to or one hundred times that of the electrons, respectively. These are minimum values which will increase if subsequent higher resolution data show the radio components to be more compact. For galaxy speeds as high as $2,500 \text{ km s}^{-1}$ the above inequality and these internal pressures lead to lower limits of intergalactic gas densities of 1.6×10^{-28} and $2.2 \times 10^{-27} \text{ g cm}^{-3}$. Although precise values require both refined observations and detailed models, the radio structure of these sources unavoidably requires densities of this order. The values are high but not unacceptable for the central regions of rich clusters.

Summary

We have obtained new and refined radio data which show that the radio extensions of NGC 1265 and 3C129 are double and have a very high degree of polarization, increasing with distance from the active galaxy. A third "head tail" radio galaxy has been detected in the Perseus cluster with the extension nearly towards NGC 1275. The interacting galaxies hypothesis¹ is severely strained by more recent results^{2,3}, and our data show that it is no longer tenable. The alternative we have proposed requires conditions expected to be satisfied quite naturally in rich clusters: large galaxy speeds and a high intergalactic gas density. Our interpretation of the radio data is consistent with kinematic information for such clusters and with the densities required for the extended X-ray sources. It is significant that in all three clusters discussed here the Uhuru satellite has detected diffuse X-ray emission¹⁰; add the requirement that some of the cluster galaxies must be active radio sources of moderate power and we have a set of conditions sufficient for the phenomenon discussed to arise. Bridle and Feldman point out (personal communication) that these clusters also have very steep spectrum radio components which are probably extended sources as detected in Perseus¹ and Coma² and may be attributed to relativistic particles diffused out of many modestly active galaxies into the intra-cluster gas with its associated magnetic field. If our model is correct, narrow radio trails such as the one shown in Fig. 1 preserve information about the history of their galaxy's activity, spanning several hundred million years.

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LETTERS TO NATURE

PHYSICAL SCIENCES

Multiple Redshifts in QSOs

FOUR QSOs (PHL 957, Ton 1530, Parkes 0237-23, and 4C 05.34) are now known with complex absorption spectra; these have been interpreted¹⁻⁵ as objects with multiple redshifts. It is disturbing that even with the use of up to 10 redshifts per object, 20% or more of the absorption lines which are definitely present with strength 2 or greater remain unidentified.

Several possibilities may apply to these lines. Lynds⁵ has suggested that the unidentified features to the blue of the Ly α emission represent redshift systems where just Ly α appears. Lowrance *et al.*¹ propose that these are redshift systems with several lines in each system, where Ly α is not present or is very weak. This is possible if, for example, the temperature is so high that CIV is the dominant C ion, but H is so completely ionized that Ly α is unobservable. Using collisional ionization calculations^{6,7}, which may not be strictly applicable, the ratio of the optical depth of $\lambda 1549$ Å (CIV) to that of Ly α changes by a factor of 10^3 when T increases from 30,000 K to 60,000 K. Redshift systems with Ly α in the observed wavelength range, but very weak or absent, have been automatically rejected in previous studies.

A third possibility is that these lines belong to the redshift systems that have already been found, but that the lines have not been identified. Bahcall's list⁸ of expected QSO absorption lines is used in the computer programs of each of the groups which have worked on the identification of absorption lines in complex QSO absorption spectra; this raises the suspicion that perhaps the problem is that this identification list of expected lines is incomplete.

To investigate this hypothesis, I have taken each observed wavelength of the unidentified lines of strength 2 or greater in the 4 QSOs cited above, and derived the corresponding set of values of the wavelength at the source, λ_0 , for each redshift system observed in the particular QSO. This produces, for each QSO, an array of wavelengths of dimension (number of unidentified lines) $\times$ (number of redshift systems in the QSO). For each member of this array, I have looked for a line λ_0 , in another redshift system, either in the same or in a different object, which has $|\lambda_0 - \lambda_0'| \leq 1$ Å. Because these are wavelengths at the source, $\Delta\lambda = 1$ Å is equivalent to $\Delta\lambda = 2.5$ to 4 Å in the observers' frame. I found 19 such pairs (and 2 triplets) for the 150 values of λ_0 from the 20 most certain redshift systems and the 27 unidentified lines of the 4 QSOs. This is no larger than the number of pairs expected on the basis of rough probabilistic arguments, and in fact 25 pairs

are found with $1 \text{ Å} < |\lambda_0 - \lambda_0'| \leq 2 \text{ Å}$. It seems therefore that the 19 pairs observed are mostly coincidences.

I conclude that the unidentified absorption features in QSO spectra cannot be explained by adding lines to the basic identification list of Bahcall; additional redshifts must be present in the objects. Analysis of the unidentified lines of more QSOs with complex absorption spectra would strengthen this conclusion. I further suggest that the standard requirement for an acceptable redshift system, that Ly α be present if in the accessible wavelength region, should be eliminated.

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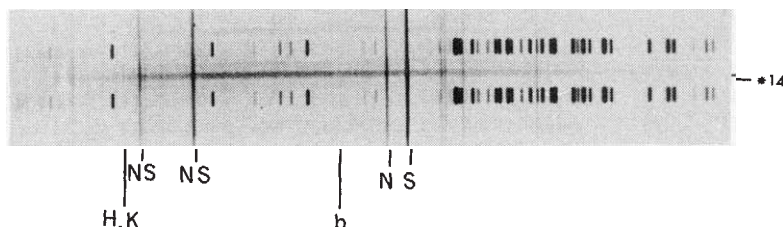
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Search for Optical Counterpart of GX3+1

AN accurate determination of the position of the X-ray source GX3+1 has been reported by Janes *et al.*¹, who used the results of lunar occultations of the source during two rocket flights. The intersecting positions of the lunar limb, as seen from the rocket, occur near the centre of the previous best position of the source determined by Schnopper *et al.*². Slitless spectra of stars in the spectral range 5600 to 8000 Å in the field of the occultation position were obtained by Kunkel *et al.*³ who found no emission features in an angular field of 7.5×2 arc min. Following a comparison of the occultation position with a Royal Observatory, Cape Town, direct photographic plate, Janes *et al.* concluded that either GX3+1 is star 14 of Kunkel *et al.*, or it is optically fainter than 18 mag. and lies between stars 14 and 15, or it is fainter than 21 mag. and lies in the occultation error box.

I have obtained two spectra with the 74-inch at Mount

Fig. 1 The spectrum of star 14 in the wavelength range 3500 Å to 6800 Å; original dispersion 200 Å mm⁻¹. Several night-sky lines (NS), some from city lights, are marked together with the strongest stellar absorption features, Ca II K and H and Mg I b.



Stromlo. The first, with 5–6 Å resolution and covering the range 3400 to 6800 Å, is of star 14 and is widened 0.3 mm. The second is not widened and, with an entrance slit width of 2.1 arc s, was guided to lie between stars 14 and 15. Deterioration of the seeing from roughly 1.5 arc s to 3 arc s during this exposure resulted in contamination of the spectrum by some light from stars 14 and 15. The spectrum of star 14 is shown in Fig. 1. It is roughly of spectral type F8V–G5V and there is no emission or any other peculiarity which might suggest a source of high thermal or non-thermal excitation. The unwidened spectrum shows no emission, but the light of star 14 dominates the spectrum. I conclude that star 14 is not the source of GX3+1 X-ray emission and that star 15 is probably not the source of this radiation. There is no evidence of line emission in the region between stars 14 and 15. It is ironic that a previous search⁴ for an optical candidate for GX3+1 based on poor X-ray position data revealed a star which, with strong emission features showing excitation temperatures of about 500,000 K, is an *a priori* candidate as an emitter of soft thermal X-rays.

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Remanent Magnetization in Meteorites

ALTHOUGH the existence of natural remanent magnetization in meteorites has been taken as evidence for a primaevial magnetic field in the original bodies—presumably of asteroidal dimensions—from which meteorites formed¹, it seems surprising that an asteroidal core (~100 km diameter as an upper limit) can sustain convective motions efficient enough to produce the magnetic field, ~10⁻¹ to 1 G, required by the NRM measurements². Moreover, recent data suggest that iron meteorites are not necessarily fragments of a much larger core, as had been supposed, but formed as small bodies embedded in a silicate matrix³. As there is no other obvious internal mechanism for generating NRM, could an external mechanism be responsible? During the early history of the solar system, the magnetic field of the Sun may have been much more intense than it is at present, possibly ~100 G (ref. 4), and the rotation of the Sun may have been much more rapid. A recent estimate for solar spin damping by the solar wind gives an e-folding time ~2.2 × 10⁹ yr (ref. 5). There are two mechanisms by which NRM could have been created in meteorites under these conditions.

First, the enhanced magnetic field and rotation rate of the early Sun imply a very greatly increased flux of magnetic induction in the ecliptic plane, which can lead to large-scale flows of current in bodies moving in this plane⁶ and so to the production of magnetic fields up to ~1 G. Solid material placed in the field will normally acquire only a soft isothermal component of NRM, but this can be fixed in small objects, or surface material, by the interplanetary neutron flux⁷. Alternatively, if the material involved was initially molten, then cooled through its Curie point, NRM might be acquired

in the usual way; but the sector nature of the interplanetary field could prevent this.

The second magnetization process depends on the occurrence of high-velocity (~5 km s⁻¹) collisions between asteroidal bodies, such as probably generated the meteoroidal fragments. These impacts can produce vaporization of the bodies in the area of contact, together with shock effects and fragmentation throughout a much larger region. A plasma is produced, which is momentarily compressed by the continuing motion of the remaining parts of the bodies. For two asteroidal bodies moving with typical cosmic velocities, the compression factor may be ~10–100. If the interplanetary magnetic field is frozen into the plasma generated, compression increases the magnetic field intensity between the two bodies. In the early solar system, a magnetic field ~10⁻¹ to 1 G could be produced briefly during a collision at 1 AU from the Sun. (This ignores the possibility of any further amplification of the magnetic field by dynamo-type effects during the collision.)

Can a short-lived magnetic field in the order of a few seconds duration produce NRM in the collisional fragments? Meteorite NRM resides in the iron–nickel fraction, and could be induced if the Curie temperature of this fraction was exceeded during the brief collisional enhancement of the magnetic field. The collision must generate a shock wave (a relative velocity of 1 km s⁻¹ leads to a shock pressure ~130 kbar (ref. 8)). Most meteorites have been subject to at least moderate shock⁹, but estimated temperatures suggest that the Curie point of iron would only be exceeded for heavily shocked specimens. Laboratory experiments indicate, however, that iron–nickel alloys have a pressure-sensitive Curie point, so that both peak pressure and peak temperature may be important (see ref. 10). The iron–nickel ratio in meteorites varies (for example, in the kamacite–taenite diffusion borders¹¹), so that some effect of shock on the Curie point seems inevitable. (The greatest values of NRM for meteorites occur for the group of iron meteorites with the highest proportion of nickel¹².) Because the Curie-point transition is related to the passage of the shock wave, its duration is appropriately brief (~1 s). The passage of the shock can have an ordering effect on magnetic domains, so that NRM production by shock may be more efficient than straightforward cooling through the Curie point.

The methods of producing NRM described here could also apply to the Moon. In the case of impact-produced NRM, the depth below the crater to which material may be affected is ~1/10 of the crater diameter¹³. Ejected material would, of course, show NRM in a similar manner to meteorites.

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Lunar Influence on Atmospheric Ozone

EVIDENCE has been put forward for a lunar influence on different geophysical data such as rainfall^{1,2} freezing nuclei counts³, radar meteor⁴, geomagnetic disturbances⁵, twilight sky brightness⁷, atmospheric ozone⁶, and sky light polarization⁸. I have analysed ozone data for twelve stations, six from each hemisphere, and the results are reported here. Ozone data used are those published by the World Meteorological Organization in cooperation with the Meteorological Service of Canada.

Table 1 Daily Total Ozone Data Utilized

No.	Station	Latitude	Longitude	Period of record
1	Aarhus	56 N	10 E	Jan. 1960–Dec. 1967
2	Goose Bay	53 N	60 W	Jan. 1962–Dec. 1967
3	Arosa	46 N	9 E	Jan. 1960–Dec. 1967
4	Toronto	44 N	79 W	Jan. 1960–Dec. 1967
5	Tateno	36 N	140 E	Jan. 1960–Dec. 1967
6	Ahmedabad	23 N	72 E	Jan. 1960–Dec. 1967
7	Kodaikanal	10 N	77 E	Jan. 1960–Dec. 1967
8	Gan	0.4 S	73 E	Jan. 1964–Oct. 1967
9	Huancayo	12 S	75 W	Feb. 1964–Dec. 1967
10	Brisbane	27 S	153 E	Jan. 1960–Dec. 1967
11	Aspendale	38 S	145 E	Jan. 1960–Dec. 1967
12	Port Aux Francais	49 S	70 E	Jan. 1960–Dec. 1967

Table 1 presents a list of stations, their geographic coordinates and the period of record. Ozone data for each of the stations are arranged according to the lunar days for the entire period of study (date on New Moon represents lunar day 0). The values for the same lunar day are averaged over different lunations. The departures of these averages from the lunar monthly mean are divided by their standard deviations, and the ratios have been subjected to harmonic analyses so that amplitudes and phases of different harmonics are determined. This has revealed two major periodicities, with periods of semi-lunar month and lunar month.

Table 2 Amplitudes and Phases of Lunar and Semi-lunar Cycles in Ozone and Variances explained by Each Component

Station	Lunar cycle		Semi-lunar cycle		Variance (%)		
	Amp- litude	Phase (days after New Moon)	Amp- litude	Phase (days after New Moon)	Lunar	Semi- lunar	Total
Aarhus	0.92	24.9	0.57	1.4	42.3	16.2	58.5
Goose Bay	0.76	21.8	0.71	1.1	28.9	25.2	54.1
Arosa	0.38	18.3	0.90	2.8	7.2	40.5	47.7
Toronto	0.57	22.3	0.47	-1.4	16.2	11.0	27.2
Tateno	1.25	13.5	0.47	-1.8	78.1	11.0	89.1
Ahmedabad	0.41	3.7	1.13	1.2	8.4	63.8	72.2
Kodaikanal	0.90	26.8	0.50	4.4	40.5	12.5	53.0
Gan	0.67	28.7	0.98	-1.3	22.4	48.0	70.4
Huancayo	0.74	21.4	0.36	9.8	27.4	6.5	33.9
Brisbane	1.13	25.0	0.43	9.2	63.8	9.2	73.0
Aspendale	1.02	22.5	0.93	7.4	52.0	43.2	95.2
Port Aux Francais	1.00	22.8	0.50	-1.7	50.00	12.5	62.5

Table 2 presents amplitudes and phases (days after New Moon) of these oscillations in ozone at all the above stations, and the variances explained by the two components of lunar oscillations. Clearly the maximum amplitude of lunar cycle in ozone occurs around the last quarter and the cycle is weak at Arosa and Ahmedabad. The semi-lunar component of ozone cycle gives maxima near the New Moon and the Full Moon at all the stations except Huancayo, Brisbane and Aspendale, where maxima occur around the first and the last lunar quarters. The semi-lunar cycle in ozone is very weak at Huancayo and Brisbane.

No systematic variation with latitude of either amplitude or phase of these two oscillations is evident. But most of the variance can be explained by these two components alone.

Adderley⁶ reported from analyses of ozone data at Arosa and Canberra that the amount of ozone increases at the vernal equinox about the first and the third lunar quarter and decreases at the same lunar phases at the autumnal equinox, with no effect at mid-winter or mid-summer.

The tidal influence of the Moon on the atmosphere, acting on the scattering particles present in the atmosphere, has been suggested as the cause of the cyclic variations in sky brightness⁷ and in maximum sky polarization⁸. It is possible that this dust might destroy ozone in the atmosphere in the manner suggested by Pittcock⁹ to explain a "hole" in ozone profile near 50 mbar level over Fort Collins, producing the observed variations. But this explanation is speculative and requires further investigation of more ozone records at many places before it can be established.

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Rate of Horizontal Fault Displacement in New Zealand

ACCORDING to theories of plate tectonics¹, a band of concentrated horizontal strain extends from a triple-point junction near 60° S 158° E through New Zealand to a triple-point junction near 2° S 128° E. The region of the band of strain within New Zealand is known as the Alpine Fault Zone². From seafloor spreading rates and from the known horizontal strain directions on the Alpine Fault Zone, the rate of horizontal strain is estimated to be about 34 mm per year^{3,4}. In the southern part of New Zealand, the fault zone is represented by the dextral Alpine Fault itself, and in the northern part by several sub-parallel dextral faults that branch off from the Alpine Fault (Fig. 1).

The position of active faults is shown by the displacement of topographic features and is best measured by the displacement of flights of terraces⁵. The faulted terraces have been eroded in late Quaternary gravels, and step down from an aggradational surface at the top of the gravels to the flood plain of the present day river and so they provide a cumulative record of the vertical and strike-slip displacements back to the formation of the aggradation surface. A comparison of the rates of horizontal strain inferred from seafloor spreading and from the displacement of the faults is necessary to know the age either of the aggradation surface itself or of one of the younger terraces.

No material suitable for radiocarbon dating has been found in any of the faulted river terraces, and it has been assumed that cold conditions would lower the forest line, increase erosion, and cause river aggradation. The various ages that have been proposed for the aggradation surface, which range from 35,000 to 15,000 yr, are thus mostly those of the cold periods of the late Pleistocene as defined in the northern hemisphere⁶. It now seems that the correct age is ~10,000 yr, and that river aggradation was determined not by the height of the forest line

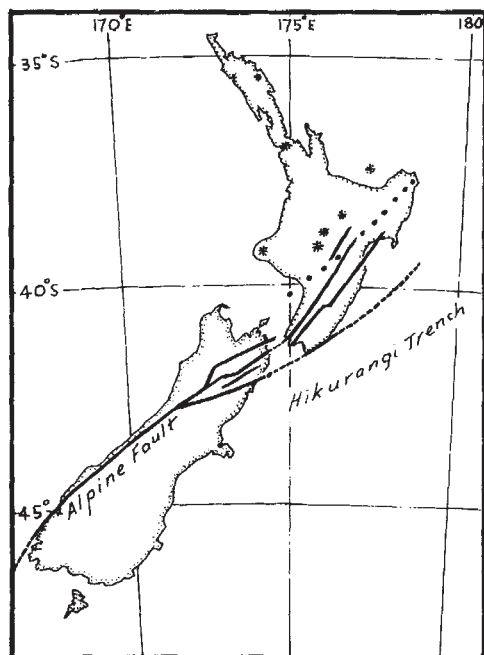


Fig. 1 Map of New Zealand. —, Main active faults of the dextral Alpine Fault Zone; . . . , axis of negative isostatic gravity anomaly; *, volcanoes active since AD 1. The approximate position of the Hikurangi Trench is also marked.

but by a change in a climatically determined erosion regime⁶. Surface smoothing and gully infilling are thought to have taken place in the central part of New Zealand during the cold periods of the late Quaternary⁷, and I now suggest that the change to the present regime of gully erosion started suddenly approximately 12,000 yr ago, when northern hemisphere temperatures first reached values close to those of the late Holocene and the present day, following the most recent glaciation⁶.

At Waiohine River, 60 km north-east of Wellington City, eight terraces⁴ are displaced vertically and dextrally by the Wairarapa Fault (Table 1, Fig. 2).

The Wairarapa Fault lies on the south-east side of the Rimutaka Range. Near Turakirae Head 40 km south-west of Waiohine River (Fig. 3), where the fault crosses the south coast of North Island, there are five uplifted and tilted beach ridges⁸. The greatest uplift (27 m) is at the axis of the Rimutaka Range; the most recent axial uplift (2.5 m) was during the 1855 earthquake.

The highest beach ridge was formed when the average rate of tectonic uplift first exceeded the rate of rise of eustatic sea level, and from the average rate of rise determined from six eustatic sea level curves⁹ the highest ridge seems to be roughly 6,300 yr old. Radiocarbon dating of samples from beds a few metres below the highest beach ridge supports this estimate¹⁰.

The uplift of each of the five beach ridges at the range axis, and the ratio of each beach ridge uplift to the corresponding vertical faulting at Waiohine River, are given in Table 1. The uniformity of the ratios is explained if the uplifted beach ridges and the river terraces were formed by the same series of earthquakes.

If the five youngest river terraces are thus correlated with the five uplifted beach ridges, the oldest of these five terraces must be 6,300 yr old. Ages for the other terraces, estimated on the assumption that the average rate of uplift, the average rate of vertical faulting, and the average rate of strike-slip faulting have been uniform, are also shown in the Table. The value of 123.2 for the horizontal displacement of a (the Waiohine Surface) is an inferred value based on the ratio between the strike-slip and vertical displacement for terrace b.

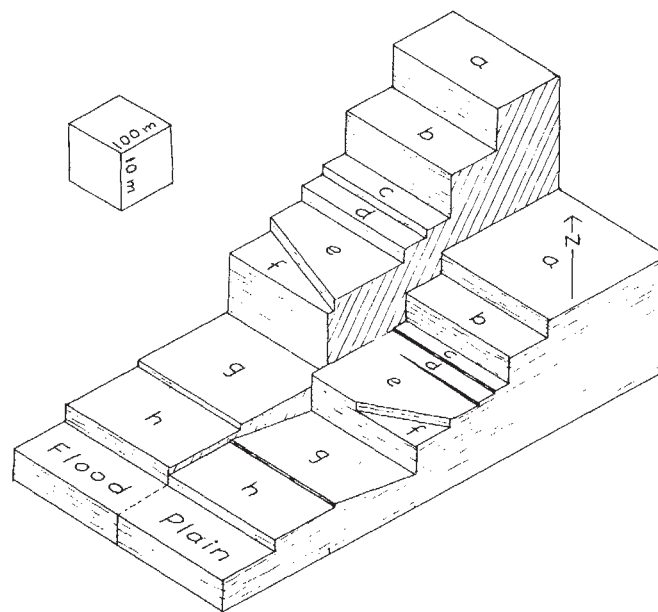


Fig. 2 Isometric block diagram of terraces at Waiohine River faulted by Wairarapa Fault. Idealized from Fig. 2 of ref. 5 with ten-fold vertical exaggeration. Note that strike-slip displacement is indeterminate for terrace e and that vertical and strike-slip displacement are both indeterminate for terrace f.

The estimated age for the Waiohine Surface is roughly 10,000 yr, considerably less than earlier estimates. According to the inferred age the average rate of strike-slip displacement on the Wairarapa Fault is about 12 mm yr⁻¹. The full horizontal displacement rate is unlikely to be much greater, because the dip of the fault plane is probably greater than 45°.

Two other important dextral faults at the south end of North Island are the Wellington Fault and an unnamed fault off the south-east corner of the island (Fig. 3). The total rate

Table 1 Comparison of Beach Ridge Uplift and Waiohine River Faulting

Turakirae Beach			Waiohine River		Inferred age (10 ⁻³ yr)			
Ridge	Cumulative uplift U (m)	U/V	Terrace	Cumulative vertical displacement V (m)	Cumulative strike-slip displacement H (m)	U × 6.3/27.0	V × 6.3/12.0	H × 6.3/66.9
B	2.5	2.8	h	0.9	12.0	0.6	0.5	1.1
C	9.0	2.5	g	3.6	32.1	2.1	1.9	3.0
D	18.0	*	f	*	*	4.2	*	*
E	24.0	2.3	e	10.2	*	5.6	5.7	*
F	27.0	2.3	d	12.0	66.9	(6.3)	(6.3)	(6.3)
			c	12.3	85.5	—	6.5	8.0
			b	14.7	99.0	—	7.7	9.3
			a	18.3	(123.2)	—	9.6	(11.5)

* Not determined.

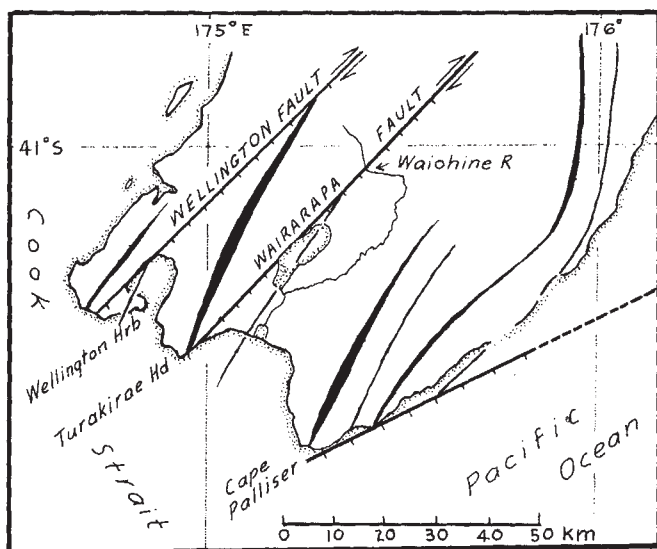


Fig. 3 Map of southern end of North Island, New Zealand. —, Growing anticlines represented by mountain ranges; - - -, growing synclines represented by topographic depressions; —, active dextral faults represented by scarps and by interrupted topography.

of horizontal strain at the south end of North Island is thus probably at least 24 mm yr^{-1} . At the north end of South Island the chief dextral faults are probably all on land (Fig. 1) and according to the observed faulting² the total horizontal displacement since the formation of the aggradation surfaces equivalent to the Waiohine Surface is probably roughly 240 m. The rate at the north end of South Island is thus roughly 24 mm yr^{-1} . Some horizontal strain is likely between the faults, and the fault rates are in reasonably good agreement with the 34 mm yr^{-1} rate inferred from seafloor spreading.

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Cyclic Temperature Oscillations from 0–20,300 yr BP

DURING the late and post Pleistocene, cyclic oscillations in global temperature seem to have occurred, superimposed on the predominant temperature trends determined by the advance and retreat of the mid-latitude ice sheets. Oscillation periods of around 2,600 yr (refs. 1–3) and of around 400 and 2,400 yr (ref. 4) have been proposed. Since these cycles were noted further palaeotemperature data have been obtained and are here included in an analysis of cyclic temperature patterns over the past 20,000 yr.

To minimize bias in the selection of data from the chronologic material, I combined the results of seven summaries of glacial chronology and of the two detailed terrestrial ^{18}O palaeotemperature studies^{5,6}. Values listed in Table 1 are my interpretation of the timing of their successive maximum and minimum values. Glaciologic data were drawn from several summaries^{7–12}, and glaciologic dates are the culmination point of advances or recessions, regardless of absolute glacial magnitude.

The values in Table 1 show considerable similarity in the chronologic trends presented by each of the palaeotemperature studies and by the glaciologic data. The palaeotemperature data show a nearly identical sequence of maxima and minima and have a series of paired values which are similar in sign and separated by a mean of 290 yr until 12,400 BP when the Hendy curve becomes less precise. The similarity in the patterning of the data in Table 1 is shown by a "run test" (ref. 15), which gives a value of $z = -1.67$ ($P < 0.047$) and indicates that this sequence is not random. Such lack of randomness supports the hypothesis of global climatic change.

Because the "run test" indicates a non-random sequence, I made tests on the data in Table 1 by a χ^2 analysis against cycles of from 100 to 2,800 yr, at 25 yr intervals (a total of

Table 1 Palaeotemperature and Glaciologic Data, 20,300 yr BP to Present

Date 1,000 years BP	Trend	Reference	Date 1,000 years BP	Trend	Reference
0.2	—	10, Fig. 4	10.7	—	7, p. 51
0.225	—	13, 5 "a"	10.9	—	5, Fig. 4
0.4	—	5, Fig. 4	11.0	—	7, p. 94
0.6	—	6, Fig. 53	11.1	+	6 Fig. 51
0.75	—	13, 5 "b"	11.3	+	14
0.8	+	10, Fig. 4	11.4	+	12, p. 228
0.9	+	6, Fig. 53	11.6	+	5, Fig. 4
0.9	+	5, Fig. 4	11.8	+	7, p. 94
1.3	—	10, Fig. 4	11.8	—	11, Fig. 11
1.375	—	13, 5 "c"	11.95	—	12, p. 228
1.4	—	6, Fig. 53	12.0	—	5, Fig. 4
1.5	—	5, Fig. 4	12.1	—	6, Fig. 51
1.7	+	6, Fig. 53	12.25	+	7, p. 39
1.8	+	10, Fig. 4	12.25	+	12, p. 228
1.8	+	5, Fig. 4	12.4	+	5, Fig. 4
2.3	—	5, Fig. 4	12.9	—	7, p. 94
2.55	—	13, 5 "d"	13.0	—	7, p. 39
2.7	—	10, Fig. 4	13.1	—	7, p. 94
2.8	—	6, Fig. 53	13.3	—	7, p. 39
3.3	+	5, Fig. 4	13.4	+	7, p. 94
3.5	—	5, Fig. 4	13.6	—	5, Fig. 4
3.8	+	6, Fig. 53	13.8	—	7, p. 94
4.1	—	6, Fig. 53	13.9	—	11, Fig. 11
4.4	+	5, Fig. 4	14.0	—	7, p. 94
4.5	+	6, Fig. 53	14.0	—	7, p. 39
4.6	—	10, p. 177	14.1	+	5, Fig. 4
4.9	—	5, Fig. 4	14.1	+	7, p. 51
5.0	—	8, p. 115	14.4	—	7, p. 94
5.1	—	6, Fig. 51	14.5	—	5, Fig. 4
5.8	+	6, Fig. 51	14.7	+	7, p. 94
5.9	+	5, Fig. 4	14.7	—	7, p. 51
6.2	—	5, Fig. 4	14.8	—	6, Fig. 51
6.5	+	5, Fig. 4	14.9	+	5, Fig. 4
6.6	—	6, Fig. 51	15.0	+	7, p. 51
6.7	—	8, p. 115	15.6	—	11, Fig. 11
6.85	—	13, 5 "f"	15.7	—	7, p. 51
7.1	+	6, Fig. 51	16.1	+	6, Fig. 51
7.8	—	11, Fig. 11	16.3	+	7, p. 51
7.9	—	6, Fig. 51	17.0	—	7, p. 51
8.0	—	9, p. 1123	17.2	—	11, Fig. 11
8.4	—	5, Fig. 4	17.6	—	6, Fig. 51
8.5	+	6, Fig. 51	18.0	+	7, p. 51
8.8	+	5, Fig. 4	18.3	+	6, Fig. 51
9.1	—	6, Fig. 51	18.5	—	7, p. 51
9.3	—	5, Fig. 4	19.1	+	7, p. 51
9.6	+	5, Fig. 4	19.6	—	6, Fig. 51
9.7	+	6, Fig. 51	19.6	—	7, p. 51
10.5	—	6, Fig. 51	19.7	—	11, Fig. 11
10.65	—	12, p. 228	20.3	+	7, p. 32
10.7	—	11, Fig. 11			

+, Temperature maximum or glacial retreat; —, temperature minimum or glacial advance.

109 cycles). Each of these cycles was then tested with division points between periods starting at every 100 yr from 0 to 800 yr BP for a total of 872 comparisons. The results of this analysis show that the maximum χ^2 values occur in a pronounced peak from 1,275 to 1,350 yr with a maximum χ^2 value of 27.3 ($P < 0.001$) for a cycle of 1,325 yr at a division point of 300 yr BP. I had previously fitted a cycle of around 1,300 yr by inspection¹, but had abandoned it in favour of a cycle of twice that length because there was a lack of information supporting a 1,300 yr cycle in some parts of the interval 13,700 BP to the present. Such information is now available, in part supplied by the two terrestrial ¹⁸O temperature studies. The possibility of a climatic cycle of around 1,300 yr is supported by studies of cycles of 1,000–1,200 (ref. 16), 1,265 (ref. 17), 1,400 (ref. 18) and 1,100–2,000 (ref. 19) yr.

If the successive peak χ^2 values for the data in Table 1 are considered, regardless of level of significance, then for the interval 100 to 2,800 yr there are 32 peaks: 125 (2.2), 175 (1.7), 225 (2.9), 300–325 (3.7), 375 (2.9), 425 (1.2), 500 (2.6), 575 (1.7), 650 (14.6), 725 (3.3), 775 (2.2), 825 (3.3), 875 (6.3), 950 (11.9), 1,025 (3.3), 1,075 (3.7), 1,150 (7.4), 1,325 (27.3), 1,525 (4.9), 1,625 (2.0), 1,700 (2.0), 1,750 (2.9), 1,825 (3.3), 1,925 (2.6), 1,975 (2.2), 2,025 (4.4), 2,150 (9.1), 2,350–2,375 (3.7), 2,475 (5.8), 2,625 (4.4), 2,700 (5.3) and 2,750 (4.9) yr (χ^2 values in parentheses, $df=1$). The two highest peaks in this sequence, 650 and 1,325 yr, may be part of a geometric progression, with a preceding term at 300–325 yr, which is the highest χ^2 value from 100 to 650 yr. The next succeeding term in the progression would be the peaks around 2,625 to 2,750 yr with a maximum at 2,700 yr.

The high degree of significance for the 1,325 yr peak suggests that a cycle of this length might be helpful in interpreting studies of late and post-Pleistocene glacial chronology and perhaps in glacial classification schemes. Table 2 shows the 1,325 yr cycle compared with a compilation of glacial chronologic dates from 0–20,300 yr BP (total 226 dates). Each glacial event is represented by only one date, though many ¹⁴C

measurements may have been averaged to ascertain the date. These data were divided into North America, Eurasia and the Southern Hemisphere to give a demonstration of the similarity of glacial patterning on a global scale. A total of 170 of the 226 dates were in accord with the 1,325 yr cycle ($\chi^2=57.6$, $P < 0.001$).

The close correspondence between the glacial dates and the 1,325 yr cycle can be seen by inspection of the basic data which shows that, when a glacial date falls outside its predicted interval, it usually does so by a small margin. Because data on glacial advance are more readily available than for recession, the glacial advance phases are represented by more dates (198) than the recession phases (28). There was usually a greater frequency of glacial advances at every other phase, that is, at 0.0–0.3, 2.3–2.95, 4.95–5.6 $\times 10^3$ yr and so on. The greatest glacial effects occurred therefore around every 2,600–2,700 yr, as noted in ref. 1. Nevertheless, there is evidence of glacial resurgence at every colder interval of the 1,325 yr cycle. The pattern of alternating periods of glacial advance and recession which has been well documented for the Bölling (12.2–12.8 $\times 10^3$ yr BP), Older Dryas (11.7–12.2), Alleröd (11.0–11.6) and Younger Dryas (10.3–10.9) intervals has been present apparently throughout the past 20,000 yr. Further studies are needed to verify this pattern for intervals with meagre data. This applies particularly to the predicted glacial advance phase around 8.9–9.5 $\times 10^3$ yr BP for which evidence has only recently become available^{9,20}.

When it is possible to convert the ¹⁴C dates used in Tables 1 and 2 to true time, then it is likely that the chronologic boundaries which resulted from the χ^2 analysis of the data in Table 1 will require alteration. Such a conversion was attempted² for the past 8,000 yr and the results of this conversion applied to the present study suggest that a date of 0 to 0.6 or 0.7 $\times 10^3$ yr BP for the recent cold phase would give a better fit to the corrected ¹⁴C dates in relation to the 1,325 yr cycle. Since the recent cold phase which culminated in the "Little Ice Age" occurred from around 1300 or 1400 AD to

Table 2 Global Glacial Oscillations, 0–20,300 yr BP, in Relation to a 1,325 yr Cycle

Period 1,000 yr BP	Glacial trend *	n	North America		Recorded oscillations			Southern Hemisphere		
			Range	Mean	n	Range	Mean	n	Range	Mean
0 – 0.3	A	59	0.0– 0.5	0.2	14	0.1– 0.5	0.3	12	0.1–0.3	0.2
0.31– 0.96	R	2	0.7– 0.8	0.8						
0.97– 1.62	A	13	0.7– 1.8	1.3	3	0.7– 1.4	0.9	3	0.8–1.5	1.0
1.63– 2.29	R	1		1.8						
2.3 – 2.95	A	10	2.3– 2.8	2.6	1		2.5	5	2.2–2.9	2.6
2.96– 3.61	R							1		3.2
3.62– 4.27	A	3	3.3– 3.7	3.4	1		3.8	2	3.7–4.2	4.0
4.28– 4.94	R									
4.95– 5.60	A	10	4.7– 6.1	5.2	2	4.6– 4.8	4.7	1		4.6
5.61– 6.26	R	1		6.1						
6.27– 6.92	A	6	6.5– 6.8	6.6	1		6.8			
6.93– 7.59	R	2	6.8– 7.4	7.1						
7.6 – 8.25	A	4	7.7– 8.3	8.0	2		7.8	1		8.5
8.26– 8.91	R	1		8.5	1		8.7			
8.92– 9.57	A	2	9.2– 9.3	9.3	1		9.2			
9.58–10.24	R	2	9.8–10.2	10.0						
10.25–10.9	A	5	10.4–11.0	10.8	3	10.5–10.7	10.6	1		11.0
10.91–11.56	R	3	11.3–11.8	11.6	1		11.4			
11.57–12.22	A				4	11.8–11.9	11.9			
12.23–12.89	R	1		12.2	1		12.2			
12.9–13.55	A	7	12.6–13.3	13.0	1		13.2			
13.56–14.21	R	1		13.4	1		13.4			
14.22–14.87	A	5	13.8–14.7	14.2	1		13.9	1		14.2
14.88–15.54	R	3	14.1–15.0	14.6				1		15.2
15.55–16.2	A	3	15.6–15.7	15.6	1		15.6	1		16.5
16.21–16.86	R	1		16.3						
16.87–17.52	A	1		17.0	1		17.2			
17.53–18.18	R	1		18.0				1		17.5
18.19–18.85	A	3	18.0–18.5	18.2	1		18.2	1		18.6
18.86–19.51	R	1		19.1						
19.52–20.17	A	1		19.6	1		19.7			
20.18–20.84	R	1		20.3						

* A, glacial advance; R, glacial retreat.

around 1900 AD, this correction is reasonable. The results of a correction from ^{14}C to true time might shift the length of the proposed 1,325 yr cycle by up to ± 50 yr, but the existence of regular temperature oscillations would probably still be clear.

The demonstration of a statistically significant pattern in the glacial oscillations in Table 2 indicates the same temporal patterns occurred for the expansion and contraction of both the large scale late Pleistocene ice sheets and for the smaller scale post-Pleistocene polar and alpine glaciers. Further, the post-Pleistocene glacial periods follow in regular phase from those of the late Pleistocene, which suggests that the timing of both major and minor glacial pulsations is related to a similar (probably cyclic) mechanism. By placing more emphasis on the temporal dynamics of global glacial chronologies regardless of magnitude, it may be easier to ascertain glacial causal mechanisms. Our best key to the origin of ice ages may be in a detailed study of the Neoglacial advances and especially those of the 1600 to 1900 AD advance phase.

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BIOLOGICAL SCIENCES

Release of Protein from Mouse Motor Nerve Terminals

RELEASE of catecholamines from the chromaffin granules of the adrenal medulla and from sympathetic nerve endings is accompanied by release of chromogranins, specific soluble proteins contained within vesicles containing noradrenaline. Since protein (chromogranin) release is dependent on calcium and is stoichiometrically related to the amount of catecholamine released, release is thought to be by exocytosis¹⁻³. We know no report of protein released at cholinergic synapses, but biochemical studies of isolated cholinergic vesicles have revealed that many different protein components are contained within these vesicles⁴. We report here evidence indicating that proteins are released from nerve terminals of the neuromuscular junction.

Experiments were performed on the isolated mouse diaphragm-phrenic nerve preparation at room temperature (20°–22° C). The hemidiaphragm was bathed in a small volume

of physiological saline⁵ containing D-tubocurarine (dtC) and, when acetylcholine (ACh) analysis was also to be performed, neostigmine as well. Preparations were perfused for 2 h before the experiment, to remove excess extracellular protein. The phrenic nerve was stimulated at 6.25/s at twice the contraction threshold current, with pulses lasting 0.2 ms. Stimulation periods were 20 min for a total of 7,500 stimuli. Control periods were for the same time but without concurrent stimulation. Endplate potentials (e.p.p.s) were monitored intracellularly with KCl-filled micropipettes. Samples were analysed only from those preparations in which e.p.p.s could be recorded on at least 90% of the penetrations. Protein concentrations were determined in preparation effluents by the method of Lowry *et al.*⁶. Protein efflux rate was obtained by dividing the protein concentration by the appropriate time interval (20 min) and was expressed as ng/ml/min. Polyacrylamide gel electrophoresis⁷ was used to characterize released proteins further. Pooled samples were dialysed and concentrated. The separatory gel we used was a 7.5–10% acrylamide gradient and the non-specific protein stain, Coomassie blue⁸, was used to locate proteins on the gel.

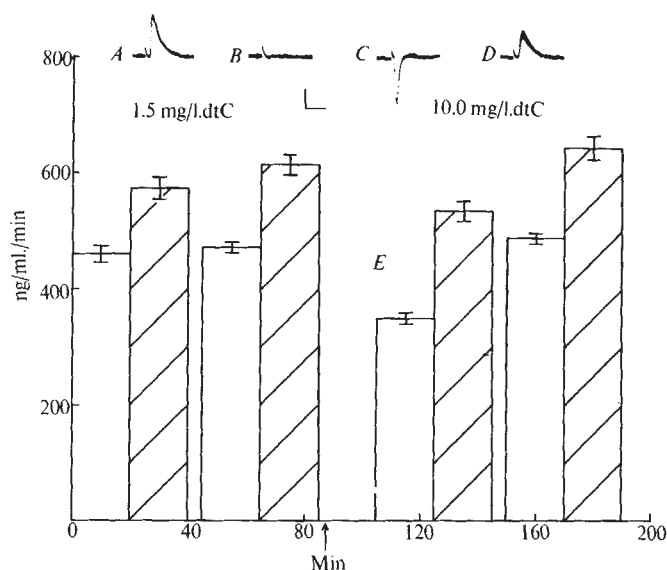


Fig. 1 Increased protein efflux produced by nerve stimulation, independent of dtC concentration. *A* to *D* are records from the same preparation in 1.5 mg/l. dtC (*A, D*) and in 10.0 mg/l. dtC (*B, C*). *A, D* show superimposed traces of e.p.p.s. before (*A*, R.P. –90 mV) and after (*D*, R.P. –80 mV) exposure to 10.0 mg/l. dtC. *B* shows no e.p.p. following the stimulus artefact (R.P. –84 mV) but nerve action potentials (*C*) could be extracellularly recorded indicating that nerve conduction was not blocked. Calibration: 1 mV and 5 ms. *E* shows protein efflux rate (ng/ml/min) from the same preparation during resting (white columns) and stimulation (hatched columns) periods. Bars indicate the mean ± 1 s.e. of four determinations of efflux rate. The $[\text{Ca}^{2+}]$ was 5 mM. The arrow on the abscissa indicates time of change from 1.5 to 10.0 mg/l. dtC.

Fig. 1 represents a typical result using the non-specific Lowry protein determination. The mean percentage increase in protein efflux (efflux rate (stimulated-control)/control $\times 100$) as a result of stimulation was $34.9 \pm 6.2\%$ (± 1 s.e.) which is a significant difference from resting efflux rates at the $P \leq 0.05$ level (Fig. 1, bar graph). A resting neuromuscular preparation liberates a significant amount of protein including most of the glycolytic enzymes^{9,10}. To test that the increased rates of protein efflux were not due to a postsynaptic action of released ACh, efflux rates were monitored in the presence of 10.0 mg/l. dtC. In these conditions it was never possible to record e.p.p.s (Fig. 1*B*). Extracellular action potentials could, however, be recorded, indicating the maintenance of nerve conduction (Fig. 1*C*). In the presence of 1.5 mg/l. dtC, e.p.p.s were consistently observed

(Fig. 1.4). In six experiments the percentage increase (mean $\pm$ 1 s.e.) in protein efflux rates when preparations exposed to 10.0 mg/l. dtC ($23.1 \pm 4.4\%$) was not significantly different from the increase in the presence of 1.5 mg/l. dtC ($23.9 \pm 4.0\%$).

To correlate the increase in protein efflux with release of ACh, the concentration of the extracellular calcium was varied and the protein efflux monitored (Fig. 2). Filled circles are means of fifteen determinations of the increase in protein efflux (%) with standard errors as indicated. As Fig. 2 shows, there was no protein release in the absence of added calcium in the bathing saline while maximal protein efflux occurred in the presence of 5 mM calcium, the same calcium concentration at which ACh release determined as quantal content is also maximal¹¹ (Fig. 2, open circles, taken from ref. 11). The dependence on extracellular calcium and the close correspondence between the increase in protein release and the increase in the mean quantal content of e.p.s.s with changes in calcium concentration is strong evidence that the extra protein released by stimulation comes from nerve terminals concurrently with ACh. The possibility that phrenic nerve axons and associated structures could be the source of extra protein release has been eliminated by experiments performed on the phrenic nerve alone; ten stimulation periods produced no significant percentage change in efflux rate (mean $\pm$ 1 s.e. = $-7.3 \pm 6.6\%$).

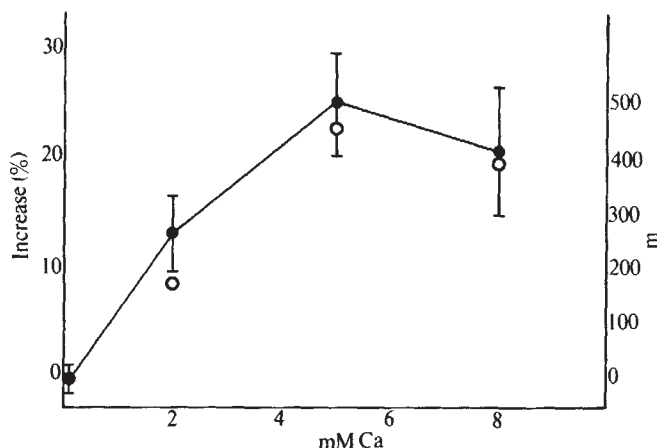


Fig. 2 Protein and ACh release as a function of extracellular calcium concentration (abscissa). The ordinate on the left shows the increased protein efflux in stimulated effluents (●). The ordinate on the right side indicates mean quantal content (○), data from ref. 11. The filled circles are the means of fifteen determinations each with its standard error (black bars).

Polyacrylamide gel electrophoresis showed that the substances detected by the Folin reaction were polypeptide in nature and not some low molecular weight peptide (Fig. 3). The left portion of Fig. 3 shows the results obtained when the tracking dye (bromophenol blue) was run to the end of the gel. To obtain better resolution, gels were sometimes run for a constant amount of time after the tracking dye had left the gel. Fig. 3 (right) is a diagrammatic representation of a gel after a 45 min post tracking dye run. The diagonal lines represent the corresponding bands on the two gels. Gels run with protein derived from stimulated preparations and those with protein from the effluent of control preparations contained approximately the same amounts of protein (100 μ g). Gels were quantitatively analysed by recording densitometry. While gels containing protein from stimulated preparations showed a greater density of staining, no consistent qualitative differences could be observed; there were no bands present in these gels that were not also in the gel with protein from unstimulated preparations. We feel the reason for this is technical. Either the proper gel conditions have not yet been used for resolution of specific released proteins or the amounts resolved are not detectable by the staining system used.

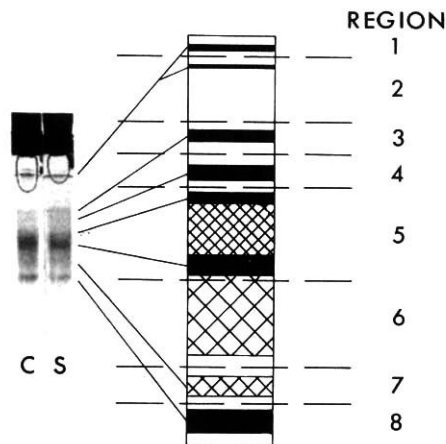


Fig. 3 Staining patterns resulting from polyacrylamide gel electrophoresis of released proteins. On the left is a photograph of the two gels from runs where the tracking dye was run to the end of the gel. The gel pattern produced by proteins collected from stimulated effluents is designated S while C is the gel containing protein from control effluents. On the right is a diagrammatic representation of the result obtained with a 45 min post tracking dye run. The diagonal lines represent corresponding bands. The dashed lines are points where unstained gels were sectioned and the resulting regions homogenized and analysed for ACh.

Preliminary results suggest, however, that the released proteins bind ACh. The dialysed substance was analysed for ACh by a quantitative chemical assay, pyrolysis-gas chromatography¹². An average of 80% of the total released ACh was found within the dialysis membrane. If concentration gradients were the only forces determining the equilibrium distribution of ACh, it would be expected that only 0.5% of the total ACh would be contained within the dialysis membrane. (The dialysed substance was in a 10 ml. total volume, while the buffer volume was 2 l.) This result suggests that a macromolecule(s) was binding ACh and preventing its diffusion out of the dialysis membrane. Furthermore, gels resulting from a 45 min post tracking dye run were sectioned at the points indicated in Fig. 3, right. The resulting regions of the gel were homogenized in buffer and the supernatant analysed for ACh by pyrolysis-gas chromatography. Results of two experiments show that region 5 of gels containing protein from stimulated preparations had a greater percentage of total ACh associated with it than other regions. Region 5 had 31.5 and 21.7% ACh while all other regions ranged between 3 and 15% ACh (mean $\pm$ 1 s.e. = $10.5 \pm 1.1\%$). In the gel containing proteins from effluents of control preparations, region 5 contained 16.5% of the total ACh and the mean $\pm$ 1 s.e. of all other regions was $11.9 \pm 2.1\%$.

The evidence reported here indicates that protein is being released concurrently with ACh at the neuromuscular junction. In view of the results presented and the proposed catecholamine binding role of the chromogranins in adrenergic vesicles¹³, we propose that proteins in cholinergic vesicles could serve an ACh binding function, which could play a role in the maintenance of high ACh concentrations in vesicles. The released proteins could also mediate postsynaptic effects, including trophic influences.

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Tension Transients after Quick Release in Rat and Frog Skeletal Muscles

THE elastic energy of isometrically tetanized muscle can be measured by applying stepwise shortenings of different magnitudes to one end of the muscle while recording the resulting drop in tension. According to A. F. Huxley's¹ sliding filaments model this energy should reflect a property of the so-called cross-bridges between the actin and myosin filaments. We have measured this with a technique which enabled us to obtain the shortening of a tetanized muscle over a distance of 0.5 mm within 0.3 ms; smaller shortenings were achieved even faster. Resonance of the force transducer occurred at frequencies higher than 7 kHz, and stiffness of the whole apparatus was 10 g μm^{-1} . The sudden length-change induced an oscillation of approximately 4 kHz in the moving part of the apparatus. This oscillation can easily be discriminated as an artefact in the force recordings.

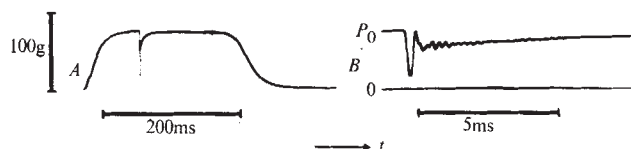


Fig. 1 Tension recordings of rat anterior gracilis muscle. Temperature 25° C, length 28 mm, weight 106 mg. Supramaximal stimulation at 100 pulses s^{-1} during 200 ms. Shortening 130 μm , occurring 84 ms after beginning of stimulation. $P_0=74$ gf. A, Total tension recording; B, extension of the same recording from 83 to 93 ms after beginning of stimulation.

Shortly before each measurement the tissue chamber with bathing fluid was removed, to prevent artefacts from movement of the surrounding fluid, induced by the displacement mechanism. Some adhering fluid provided electric contact between the muscle and the stimulus electrodes. A characteristic recording of the force of the anterior gracilis muscle of the rat at 25° C (shortening of 130 μm) is shown in Fig. 1A. Here, as in all other recordings, muscle was at resting length. In Fig. 1B, the essential part of the same recording is shown with a higher time resolution. The gradual redevelopment of force is preceded by a fast transient. Later on the above mentioned 4 kHz artefact is seen. Fig. 2 shows a series of recordings of the soleus muscle of the rat at 30° C at different shortenings. A damped oscillation of the muscle force is observed. The period of this oscillation lengthens with increasing shortening.

From the mass of the soleus muscle and its stiffness after the shortening we calculated the dominant oscillation frequency to be expected theoretically. The muscle was assumed to behave like a homogeneous rod with evenly distributed stiffness and mass. The shortening then causes a longitudinal wave which is propagated over the muscle and reflected at the ends; lowest resonance frequency (in Hz) in such a system will be: $f_r = 1/2\sqrt{(S/m)}$, where S denotes stiffness, and m mass.

Table 1 supports the validity of this concept, although the

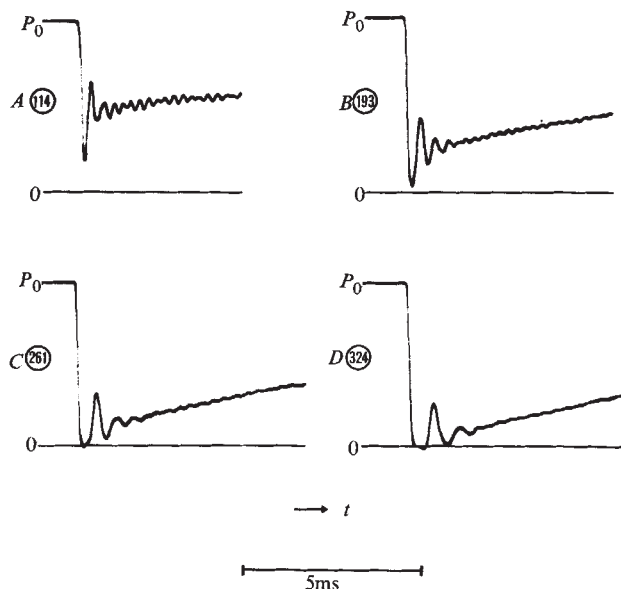


Fig. 2 Tension recordings of rat soleus muscle. Temperature 30° C, length 17 mm, weight 27 mg. Supramaximal stimulation at 100 pulses s^{-1} during 250 ms. Encircled numbers indicate shortenings in μm . P_0 in each tetanus about 38 gf. In A the 4 kHz artefact is clearly seen and interferes with the damped oscillation. As the muscle force reaches zero tension, the amplitude of the oscillation is cut off (C and D).

observed oscillation frequency was always lower than the calculated f_r . One possible cause for this discrepancy is the fact that the mass of the muscle was measured without adhering fluid.

Table 1 Dominant Oscillation Frequency of Muscle

Stiffness (N/m)	Calculated f_r (Hz)	Measured f_r (Hz)
1,200	3,400	2,800
980	3,000	2,500
770	2,600	2,200
400	1,900	1,800

In view of the popularity of frog sartorius we also performed a number of experiments on this muscle. Fig. 3 shows a response at 0° C. It should be noted that this muscle is less uniform in cross-section and has more series elasticity in the tendon than the rat anterior gracilis muscle. If this is taken into account, the result does not contradict our theories.

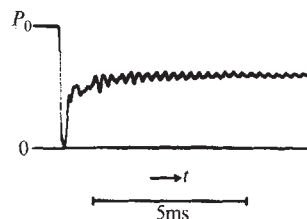


Fig. 3 Tension recording of frog sartorius muscle. Temperature 0° C, length 29 mm, weight 41 mg. Supramaximal stimulation at 20 pulses s^{-1} during 500 ms. Shortening 200 μm . $P_0=14$ gf.

We thought it useful to study the effect of a sudden shortening on a homogeneous rod with evenly distributed mass and stiffness in a passive system and in equivalent circumstances as in the experiments on muscles. We therefore designed a coiled spring with approximately the same length, mass and stiffness

as an activated rat muscle. To investigate the effect of internal friction—acting in parallel with the elasticity—on such a system we filled this spring with molasses, the viscosity of which was changed by adding different amounts of water. The effect of increasing damping is seen in Fig. 4A to D. Note the similarity between Figs. 1B and 4D.

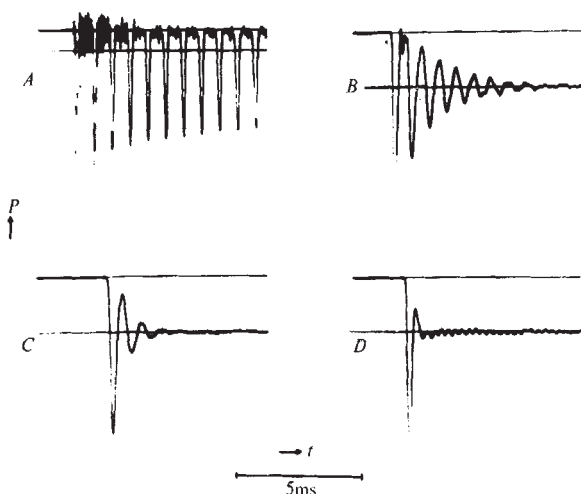


Fig. 4 Tension tracings of a coiled spring, subjected to sudden shortenings of 0.45 mm as in the experiments on muscles. Stiffness 450 N/m, mass 66 mg, length 26.5 mm. Upper horizontal line marks force exerted before the shortening, the lower one marks force some time after the shortening, when all transient phenomena were over. From A to D internal friction of the spring increases. Sensitivity of oscilloscope was 2.5 times lower for A than for B to D.

Similar tension transients were observed by Huxley and Simmons² in isolated fibres of frog semitendinous muscle at 0.5° C. As their curve appeared not completely similar to those we observed in frog sartorius muscle (whole muscle preparations)—see Fig. 3—or in the spring analogue (Fig. 4D), we looked for differences in the set-up used. One difference might be the duration of the length-change. We therefore repeated the experiment with the heavily damped spring (Fig. 4D), taking care to slow down drastically the speed of the displacement. Fig. 5 shows the result when the total length-change lasts about 1 ms. Now the response is rather similar to that shown by Huxley and Simmons.

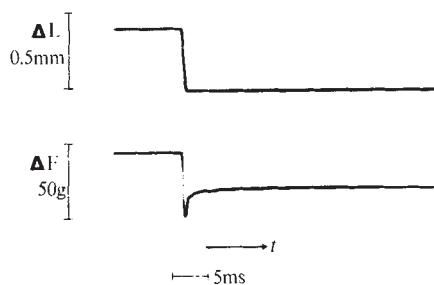


Fig. 5 Tension tracing of same heavily damped spring as in Fig. 4D. Duration of shortening about 1 ms.

Preliminary studies, using an electric analogue, have shown that several other factors may also influence the shape of the tension transients; for example, nonlinearity of, or local differences in, the friction or stiffness of the muscle, or stray compliance, for example, in tendons or connecting threads. It remains open to question, whether a linear approximation may be used in a more detailed analysis.

The observed tension transients in our studies and in those of Huxley and Simmons can thus be explained by the combined effect of mass and damped elasticity inside the muscle.

As we have pointed out³, internal friction of the muscle may act as a speed-limiting factor at high velocities of shortening as found in force-velocity measurements with small loads.

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Professor Huxley and Dr Simmons write: While we agree in the most general sense that the transients recorded by Drs Blangé, Karemaker and Kramer¹ and by us²⁻⁴ “can be explained by the combined effect of mass and damped elasticity inside the muscle”, we believe that the effect of mass in our experiments was negligible, and that in series with the highly damped elasticity there is an essentially undamped component. Thus our interpretation differs from that presented by Blangé *et al.* in two respects.

In the first place they emphasize the role of inertia in the muscle. The oscillations in their records show that this was important in their experimental conditions. Our experiments, however, were carried out on isolated fibres whose length was about one-third that of the muscles used by Blangé *et al.* The natural frequency of longitudinal oscillation will therefore have been three times higher, and no appreciable oscillation would be set up by the step changes of length that we used. In these circumstances the effects of inertia are altogether negligible, and we are justified in analysing our records as if the inertia had been zero.

In the second place, they do not postulate, as we do, an undamped elastic element in series with the damped elasticity which is evidently present. Their records from frog muscle (Fig. 3) and from their damped spring imitating frog muscle (Fig. 4D), however, both show a feature which can only be explained if an undamped (or lightly damped) element is present in series. This feature is the gradual approach of tension towards its new level during the first 1–2 ms after completion of the length change. In a uniform structure where all the elasticity is damped, the tension will come to its new level promptly at the end of the period of movement, with perhaps an oscillation above and below this level if the muscle is long. A progressive change, such as is seen in their records as well as in ours, is possible only if a redistribution of the length change is taking place between elements with widely different damping time constants.

The interesting question raised by these tension responses is the actual nature of the “damped elasticity”. The damping is not attributable to simple physical effects due, for example, to viscosity of the medium between the filaments. Any such effect would show up equally in quick stretches of resting muscle, but the effects found experimentally in resting muscle are of an altogether smaller order of magnitude than those in active muscle. We recently proposed⁴ an answer to this question, based on the highly non-linear characteristics of the responses. For the reasons stated here, we do not feel that

this proposal is in any way affected by the points raised by Blangé *et al.*

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Role of Microexudate Carpet in Cell Division

THE growth rate of cells isolated from higher organisms depends largely on population density. At low cell densities, cells have a low probability of growing in usual culture conditions (reviewed in ref. 1). Cell growth can be sustained in sparse cultures using X-irradiated feeder layers or conditioned medium. Feeder layers are thought to enhance growth by producing "conditioned factor", which seems to be restricted to the vicinity of feeder cells²⁻⁴, and is hardly diffusible into culture medium. Conditioned factor was supposed to be a kind of pericellular substance such as microexudate, which was detected by Rosenberg⁵ by using ellipsometry. While similar substances have been observed for several types of cultured cells^{6,7}, little has been known about their chemical, morphological and biological properties. We have examined the

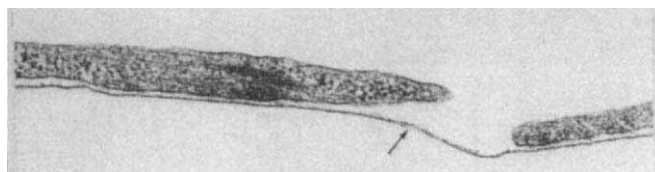


Fig. 1 Cross sections of cultured chick embryo cells. Cells were cultivated in serum-free medium and stained with uranyl acetate and lead acetate. The microexudate carpet is indicated by an arrow ($\times 25,000$).

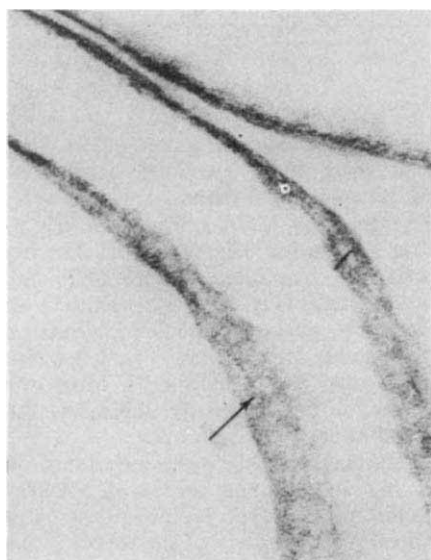


Fig. 2 Cross to tangential section of isolated microexudate carpet. An arrow indicates a hexagonal framework in the carpet ($\times 93,750$).

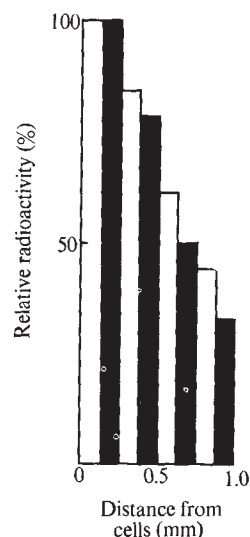


Fig. 3 Incorporation of ¹⁴C-D-glucosamine (1 μ Ci/ml.) and ¹⁴C-leucine (1 μ Ci/ml.) on coverslips incubated for 5 h at the different distances from chick embryo cells. $\square$, ¹⁴C-Glucosamine; $\blacksquare$, ¹⁴C-leucine.

microexudate carpet by electron microscopy after improving the sectioning technique for monolayer cultures of chick embryo cells.

Chick embryo cells were propagated in Eagle's minimal essential medium supplemented with 5% calf serum, if not otherwise stated. For electron microscopy, cells were inoculated in Falcon plastic dishes without calf serum. At 24 h after inoculation, cells were fixed *in situ* with 2% glutaraldehyde, postfixed with 2.5% osmium tetroxide and then dehydrated with graded alcohol. Propylene oxide was added to dissolve the surface of the dishes, and cell layers were isolated and embedded in Epon 812.

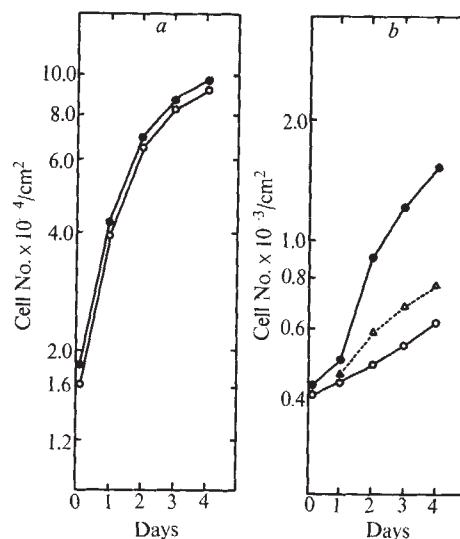


Fig. 4 Effect of the preformed microexudate carpet on growth rate of chick embryo cells in dense (a) and sparse (b) cultures. $\circ$, Control coverslips; $\bullet$, coverslips conditioned for 8 h on cell layers; $\triangle$, coverslips conditioned for 8 h on 1 mm from cells.

A thin carpet was observed under the cell layers (Fig. 1). It stained uniformly and was 70 to 100 Å thickness, although not having the trilaminar structure characteristic of cell membrane. Since the appearance of the carpet was unaffected by the omission of calf serum from the culture medium, the carpet seemed to be an aggregate of cellular products. Following Rosenberg⁵, we call it the microexudate carpet.

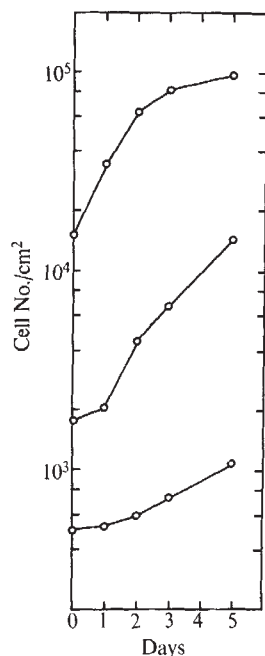
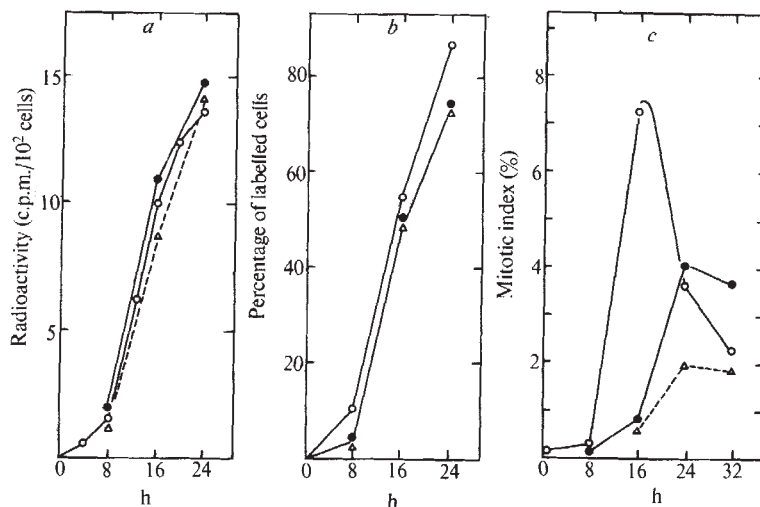


Fig. 5 Growth curves of chick embryo cells at different inoculum densities.

Fig. 6 DNA synthesis and mitosis in chick embryo cells at different cell densities. Cells in G1 were inoculated at three different densities in Petri dishes containing coverslips 3 cm across, and ^3H -thymidine (1 $\mu\text{Ci}/\text{ml}$) was added at the same time. The cells were fixed with 5% trichloroacetic acid at various times to determine the incorporated radioactivity (a), numbers of labelled cells (b) and mitotic indices (c)¹⁰. $\circ$, 1.5×10^4 cells/cm²; $\bullet$, 2×10^5 cells/cm²; $\triangle$, 4×10^5 cells/cm².



A piece of plastic plate was placed on confluent cell layers and incubated in serum-free medium for 8 h. No cell was adsorbed to the plate during the incubation, when it was placed on healthy, clear monolayers. When the plate had been fixed and dehydrated, a thin carpet was isolated from its surface by treatment with propylene oxide (Fig. 2). This carpet appeared essentially identical to those found under cell layers. There were polygonal frameworks of 300 to 400 Å in each diameter, running parallel to the plane of the carpet. The frameworks were embedded in moderately electron dense amorphous matrix. A hexagonal framework is indicated by an arrow in Fig. 2. Such a polygonal framework has also been observed in basal laminae in animal tissues^{8,9}. We have confirmed that basal lamina of cardiac muscle was constructed from hexagonal frameworks of fine filaments (unpublished). The fine structures of the carpet and basal laminae were fundamentally similar.

To examine the formation of the microexudate carpet a round coverslip 3 cm across was placed on a monolayer of cells at their late log phase to form the carpet on its surface. Radioactive D-glucosamine and leucine were incorporated linearly

into the carpet when they were added to culture medium. At 5 h, the ratios of the incorporated radioactivities on the coverslips to those in cell layers were about 5% for glucosamine and 2% for leucine respectively. Incorporations of both radioactivities were inhibited about 80% by 10 $\mu\text{g}/\text{ml}$ of cycloheximide indicating that the carpet was composed chiefly of glycoproteins.

A square coverslip was placed slantwise on a monolayer of chick embryo cells. It was supported at one end 1 mm above the cells while another end was in contact with cells. The cultures were incubated for 5 h with radioactive glucosamine or leucine. The radioactivity on the coverslip decreased as the distance between cells and coverslips increased from 0 to 1 mm (Fig. 3), indicating a gradient in the concentration of microexudate around the cells.

Round coverslips of 3 cm were conditioned for 8 h on monolayers of chick embryo cells at their late log phase. On the control and conditioned coverslips, cells were seeded at either high or low inoculum densities. On the control plates, the growth rate of cells in sparse cultures was much lower (Fig. 4b) than that in dense cultures (Fig. 4a and Fig. 5). Conditioning of coverslips enhanced the growth rate in sparse cultures markedly (Fig. 4b), but not in dense cultures (Fig. 4a). When coverslips had been conditioned for the same period at 1 mm from cells, the growth enhancement for sparse cultures was less distinct (Fig. 4b). Thus the microexudate carpet enhanced the growth of cells in sparse cultures.

In confluent monolayer cultures, chick embryo cells are arrested in G1 (ref. 10). When those cells were trypsinized and subcultured at various inoculum densities, the growth curves

could be divided into three typical patterns (Fig. 5). Cells in G1 were seeded at these three inoculum densities and DNA synthesis and mitosis were determined. At every density, synchronized synthesis of DNA started at about 8 h, lasting about 20 h after inoculation (Fig. 6a and b). The incorporated radioactivities of ^3H -thymidine per cell (Fig. 6a) and the numbers of labelled cells (Fig. 6b) were essentially the same at all densities. In sharp contrast, subsequent mitosis was delayed or reduced at the lower cell densities (Fig. 6c), consistent with the characteristic growth curves (Fig. 5). Most cells did not enter prophase at the lowest density, indicating that the cell cycle was blocked at early G2.

When small numbers of cells were inoculated on the pre-formed carpet, mitosis was enhanced, but DNA synthesis was not. Although the mechanism is unknown, the carpet plays a key role in mitosis. The property of the carpet satisfies all the prerequisites for the growth enhancing effect of feeder layer or conditioned factor; the carpet is an aggregate of macromolecules which are produced by cells and localized preferentially in the vicinity of cells. At lower cell density, longer periods would be

required for the formation of the complete carpet, resulting in the delay or inhibition of mitotic cell division in sparse cultures.

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Inhibition of Mitosis in Suspension Culture of Chick Embryo Cells

Most diploid cells isolated from animal tissues cannot grow at the suspended state and suspension culture is successful only for established cell lines or tumour cells. It would be interesting to know why primary cells can grow only when stuck on a solid substratum because transformed cells are often distinguished from normal cells by their ability to grow in soft agar suspension¹. Such a property may reflect the tendency of malignant

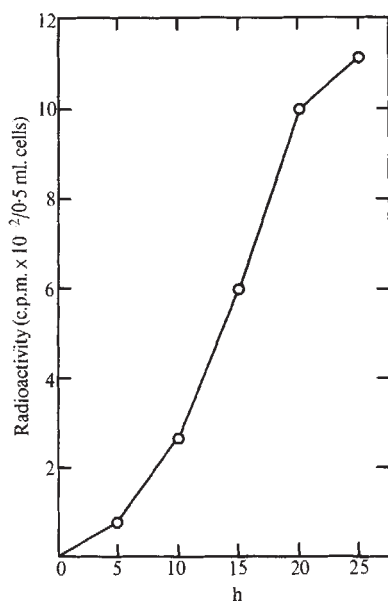


Fig. 1 Rate of DNA synthesis in suspension culture of chick embryo cells. Cells in G1 were suspended at 2×10^5 cells/ml. in suspension medium and shaken at 37°C in the presence of $1 \mu\text{Ci/ml.}$ of ^3H -thymidine. Aliquots of 0.5 ml. were taken at various time intervals to determine radioactivities incorporated into acid-insoluble fractions.

cells to be detached from original tissues and to grow freely, as in ascites tumours.

A clue to this question lies in the observation² that cultured chick embryo cells lay down a thin carpet of microexudate on the surface of Petri dishes. Without the carpet, the cell cycle is blocked in early G2, suggesting that the lack of proliferation in suspension cultures is due to failure to form the carpet. If so, the cell cycle should be blocked also at G2 in suspension cultures of chick embryo cells. We have investigated this hypothesis.

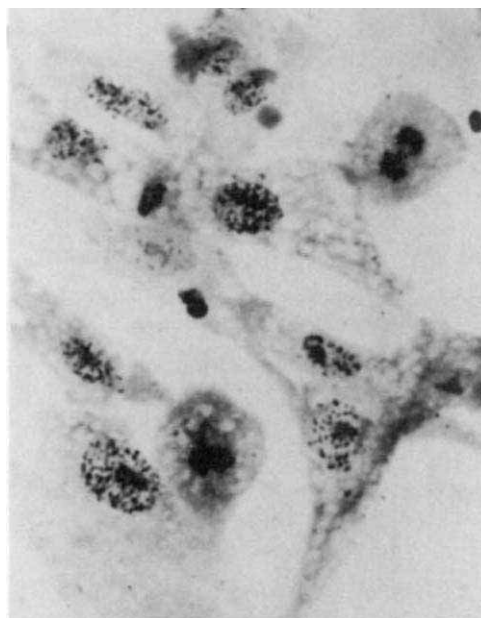


Fig. 2 Autoradiograph taken at 5 h after transfer to monolayers. Cells were preincubated with ^3H -thymidine in suspension for 24 h, as described in Fig. 1, then aliquots were transferred to Petri dishes containing coverslips. The cells were fixed at 5 h and autoradiography was carried out using Kodak NTB2 bulk emulsion. Toluidine blue was the stain.

Monolayer cultures of chick embryo cells were prepared in Eagle's minimal essential medium supplemented with 5% calf serum. For suspension cultures, Eagle's suspension medium³ was supplemented with 5% calf serum and 0.1% methylcellulose 15⁴. Cells synchronized in G1 were obtained by trypsinization of confluent monolayer cultures⁵, and suspended at 2×10^5 cells/ml. in suspension medium and shaken at 37°C . Numbers of total and viable cells did not increase in suspension cultures.

^3H -Thymidine ($1 \mu\text{Ci/ml.}$) was added initially, and aliquots were removed at various times to determine acid-insoluble radioactivities. DNA synthesis started within 10 h, lasting for about 20 h after inoculation (Fig. 1). At 24 h, an aliquot was plated in a Petri dish containing a coverslip. About 50% of cells stuck and began to stretch on the coverslip within 30 min. An autoradiograph taken after 2 h revealed that about 80% of cell nuclei were labelled with radioactive thymidine. The number of labelled cells was not changed by addition of 5 mM hydroxyurea at the time of transfer to monolayers.

Many cells entered mitosis within 4–6 h of sticking on Petri dishes (Fig. 2) and cell division continued normally thereafter (Fig. 3). When G1 cells were inoculated without preincubation, a period of 16 h was required before the first peak of mitoses appeared², indicating that DNA synthesis proceeded in suspended cells normally as well as in monolayer cells, whereas subsequent cell division was not initiated until the cells stuck on Petri dishes.

The sticking or stretching of cells on Petri dishes, however, was not enough to induce cell division; the microexudate carpet had to be formed before mitosis. When preincubated

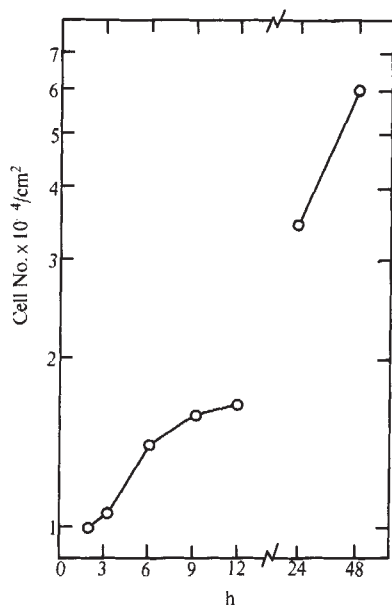


Fig. 3 Increase in cell numbers after monolayer formation. Cells in G1 were preincubated in suspension at 2×10^5 cells/ml. for 24 h, and aliquots were plated in 3.5 cm Petri dishes. After 2 h, culture medium was replaced with monolayer medium. At various times cells were trypsinized and cells were counted in a Coulter counter.

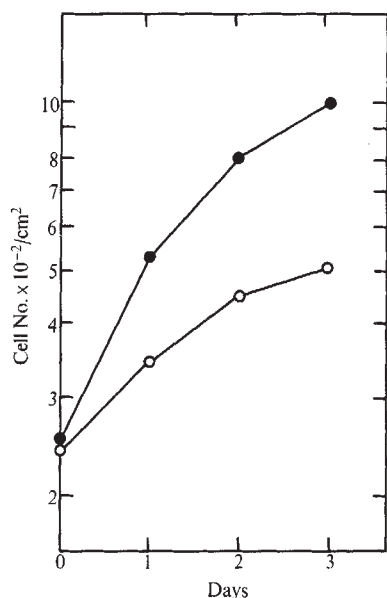


Fig. 4 Division rate at low cell density in monolayer cultures and effect of the microexudate carpet. G1 cells were preincubated in suspension for 24 h, and diluted with monolayer medium to about 2.5×10^3 cells/ml. Aliquots of 2 ml. were inoculated on round coverslips which had been conditioned for 8 h on confluent monolayers² (●), or on control coverslips (○).

cells were transferred to monolayer cultures at low cell density, about 2.5×10^3 cells/cm², the rate of division was reduced significantly (Fig. 4), while initial cell division was enhanced to the nearly normal rate by seeding the same number of preincubated cells on the preformed microexudate carpet (Fig. 4).

Clearly, the lack of proliferation in a suspension culture of chick embryo cells is due to the absence of the microexudate carpet. Environments for suspended cells differ in two ways from monolayer cells. First, the latter stick on to solid substrate, which would be an advantage for trapping the microexudate at the cell surface as a carpet. Second, cells are packed more than a thousand times more tightly in monolayers than when

the same number of cells are inoculated into the same volume of culture medium. Possibly for these two reasons, the microexudate could not be held at the periphery of suspended cells and would diffuse away into culture medium, so that mitosis was inhibited. Thus, the lack of proliferation in suspension cultures of chick embryo cells could be explained essentially by the same principle as growth restriction in single isolated animal cells².

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Vespa Larvae send out Rhythmic Hunger Signals

THE maintenance of colonies of the wasp *Vespa orientalis* F depends heavily on communication between the members. We have examined the system by which the fourth and fifth instar larvae signal to adult workers for food.

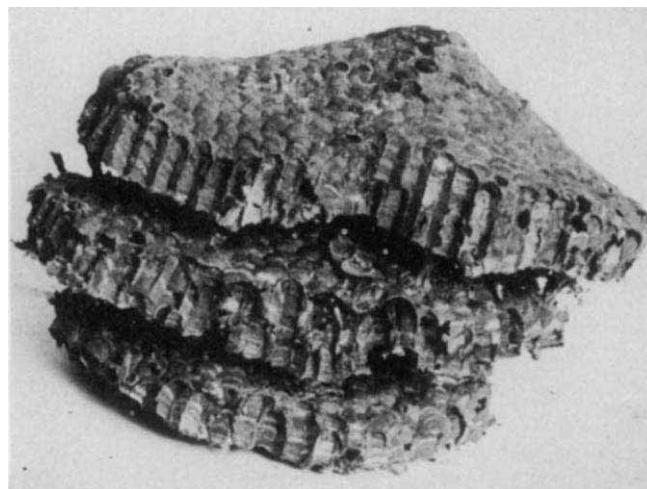


Fig. 1 A nest of larva *orientalis* consisting of three interconnected combs. The openings of the cells are directed downwards.

Vespa orientalis F is prevalent in countries of the Mediterranean basin, and builds its nest in cavities under the ground. The colony is found in the spring by a fertilized queen and grows to contain up to 4,000 individuals by the end of the summer. Fig. 1 shows the structure of a nest, composed, in this case, of three combs connected in the form of an inverted pagoda. The combs, which are constructed from transversely stratified soil "rings", are isolated acoustically from one another by longitudinal fibres consisting mainly of cellulose and soil¹. The larvae call the adult workers to come and feed them by contracting within their cells, causing their mandibles to rub against the "rings" of the cell wall and produce a scraping

sound¹. We were interested in the temporal organization of the scraping movements. We collected a small comb which contained twenty-five instar larvae and filmed their behaviour for 10 min. Examining the film frame by frame, we noted for each of six larvae, whether it was contracted or relaxed. We did not go into a more detailed analysis of the contraction itself. One larva contracted thirty times during the whole period of observation (Fig. 2).

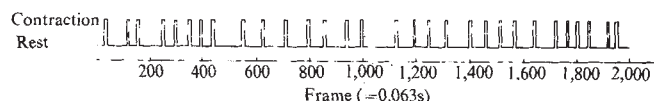


Fig. 2 The contraction cycle of a larva. The contractions of the larva are shown at their time of occurrence.

We analysed the data for rhythmicity by finding the power spectrum of the larval contractions, using a CDC 6600 digital computer and the well known BMD02T program. The results for the same larva are shown in Fig. 3. The logarithm of the power on the ordinate is plotted against the frequency of contraction (in cycles per frame) on the abscissa. There is a peak in the power spectrogram at a frequency of 0.019 cycles per frame. This peak is significant at a 2% confidence level (F -test²) and indicates that this particular larva had a tendency to contract rhythmically every fifty-three frames (3.3 s) or multiples thereof. A secondary peak can be at a frequency of 0.15 cycles per frame, denoting a cycle length of 6.7 frames (0.4 s). This peak can be attributed to the length of the contraction itself, as can be seen by examination of Fig. 2. A similar pattern of contraction was found in all six larvae similarly observed. The typical cycle length varied from eighty-three frames (5.2 s) to fifty-three frames (3.3 s). The attempt to elucidate a correlation between the contractions of these six larvae has failed so far.

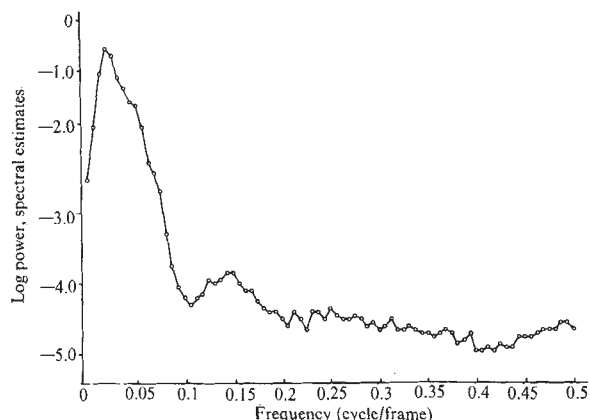


Fig. 3 The power spectrum of larval contractions. The log power spectrum estimates are plotted versus the frequency of larval contractions.

These findings may help to elucidate the code used for communication between larvae and adults in the hornet nest. The vibrations call the workers to larvae which need food and are ready to deliver to the workers newly formed carbohydrates and proteolytic enzymes in exchange (trophallaxis)^{3,4}. It may be significant that the hunger signals of the larvae are sent out rhythmically: the rhythm itself may serve as a frequency code signifying the age or the degree of need of the larva or even identifying individual larvae or groups of larvae. Work is in progress at present to evaluate these possibilities. Finally, we encounter here an example of a biological pace-

maker, where the underlying nervous mechanism still awaits elucidation.

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Artificial Insemination with Imprinted Birds of Prey

WITH the wild population of many species of birds of prey declining rapidly because of chlorinated hydrocarbon pollution and habitat deterioration, efforts to propagate raptors in captivity have increased in spite of the difficulties involved^{1,2}. I have found that, properly handled, imprinted red-tailed hawks (*Buteo jamaicensis*) can be brought into full reproductive condition in isolation from a conspecific mate, and that artificial insemination can be used for fertilization.

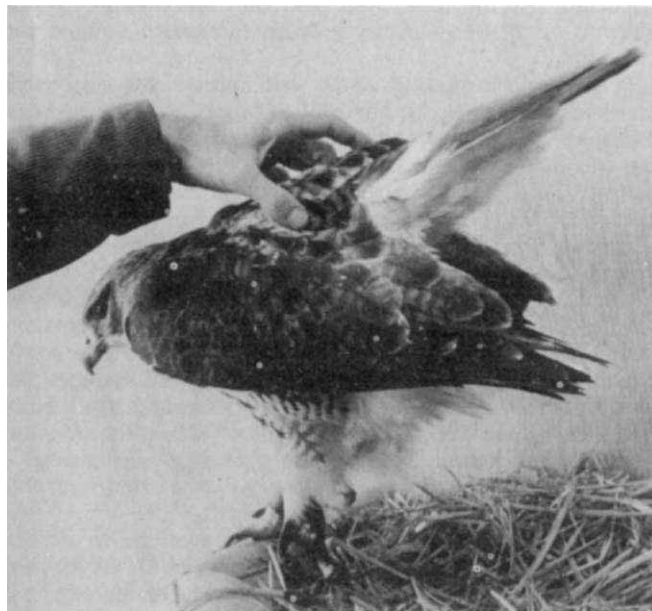


Fig. 1 An imprinted female *Buteo jamaicensis* assuming copulation posture in response to having her back stroked. Eversion of the oviduct accompanies this posture.

Many hand-reared birds become imprinted on their human handlers if isolated from their parents at an early age^{3,4}. When isolation from other members of their species continues to adulthood, these imprinted birds will often engage in social behaviour with human beings, resulting in courtship, copulation and egg-laying.

Three red-tailed hawks were used for research on imprinting and artificial insemination. One male and one female were taken from their nests as small nestlings and hand-reared in isolation from conspecifics. Another female was trapped as a mature adult. The two hawks taken as nestlings revealed imprinting early when they begged for food and interacted in other ways with their handler as they would with their parents. All birds were subsequently kept in close contact with human

beings and remained very tame. They were trained and flown free using falconry techniques and, when not being flown, were tethered to perches. The handling techniques of falconry further encouraged the hawks to accept humans as social partners.

After reaching sexual maturity the hand-reared hawks both began to direct sexual activities toward human beings; the wild female never exhibited sexual activities. By assisting the hawks in their spring nestbuilding activities and spending time daily handling them, I was able to establish a pair bond relationship with both imprinted birds. After forming a pair bond with a human, these imprinted birds rejected any conspecific mate, and aggression resulted on every encounter. The male red-tailed hawk began voluntary mounting and ejaculating on the gloved hand at 4 yr of age. The female began assuming copulation posture and everting her oviduct in response to stroking her back when she was 5 yr old. Sexual activity in both hawks was most intense during the normal spring breeding season and waned during the rest of the year. At 6 yr of age, the hand-raised female laid her first egg.

Knowing that both imprinted birds could be brought into full reproductive condition, I attempted artificial insemination during the 1971 breeding season. Semen was collected in a glass vial when the male, then 14 yr old, voluntarily copulated with the gloved hand. The semen was introduced into the oviduct of the receptive female using a glass pipette within half an hour of collection. Insemination was performed daily beginning 10 days before oviposition and continued until egg-laying ceased. Semen volume averaged about 0.1 ml. per ejaculation, if the male ejaculated only once daily. More frequent ejaculations greatly reduced the semen volume per collection.

Both eggs produced in 1971 were fertile; the eggs were removed from the female and artificially incubated in the hope that she would lay more eggs, but this was unsuccessful. One egg was incubated and hatched in a forced-air poultry incubator (37.6° C, 75% relative humidity), but the other failed to hatch after being incubated by a domestic fowl (*Gallus gallus*). The young hawk was hand-raised and appeared to be normal in growth and development.

Captive breeding of many wild birds, including the birds of prey, is difficult because the restrictions of confinement often inhibit natural pairing and mating, and normal reproductive physiology is impaired so that the gonads do not become fully functional^{6,7}. Artificial insemination with imprinted birds may allow aviculturists to breed other difficult species in captivity since imprinting has been reported in many orders of birds⁴. Artificial insemination of several other wild birds has been attempted but with limited success^{8,9}.

As wild populations of certain birds continue to decline captive propagation holds one of the few hopes for the survival of some rare and threatened species¹⁰. The techniques described in this paper may be a valuable tool for the conservationist attempting to propagate endangered species.

I thank Mr Joseph Price for providing the female hawks used in these experiments and Dr Tom J. Cade for encouraging the project.

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Relationship between Plasma Cortisol and Human Sexual Activity

No reliable information is available regarding the relationship between blood levels of adrenocortical steroids and human sexual activity. This paper attempts to provide such data by describing the results of a prolonged study in one male subject.

The subject was aged 38 yr, has been married for 11 yr and has four children. Sexual intercourse took place in normal private conditions, spontaneously and unplanned. Two daily control samples were taken for 45 consecutive days—one at 0800 h and the other at 2000 h. In addition, two types of samples were taken during coitus, the first just before orgasm (pre-orgasmic sample) and the second immediately after ejaculation (post-orgasmic sample). The specimens were placed in lithium heparin tubes and centrifuged promptly; plasma was pipetted off, deep frozen immediately and stored at -20° C. In addition, acute variations in plasma cortisol levels were investigated on two evenings over a 2.5 h period in the course of which coitus took place; during this period five or six blood samples were taken. Measurements of plasma cortisol were performed in duplicate using the competitive protein binding technique described by Murphy¹ with slight modifications. The method is highly reproducible and reasonably specific; in our hands the coefficient of variation was approximately 10%.

Cortisol levels varied markedly from one day to another. Nevertheless, there was a marked circadian rhythm, readings being considerably higher in the morning samples than in those obtained in the evenings. In morning specimens, values ranged from 3.9 to 17.0 $\mu\text{g } 100 \text{ ml}^{-1}$ (mean = 8.85 ± 0.49 s.e.m., $n=43$); in evening samples the range was from 1.2 to 7.5 $\mu\text{g } 100 \text{ ml}^{-1}$ (mean = 3.29 ± 0.29 (s.e.m.), $n=45$). The difference between the two mean values was highly significant ($P < 0.001$).

Cortisol measurements taken at the time of coitus together with relevant control estimates are shown in Fig. 1. The intervals between control and the pre-orgasmic samples averaged 145 ± 20 min (mean $\pm$ s.e.m.) and the time elapsing

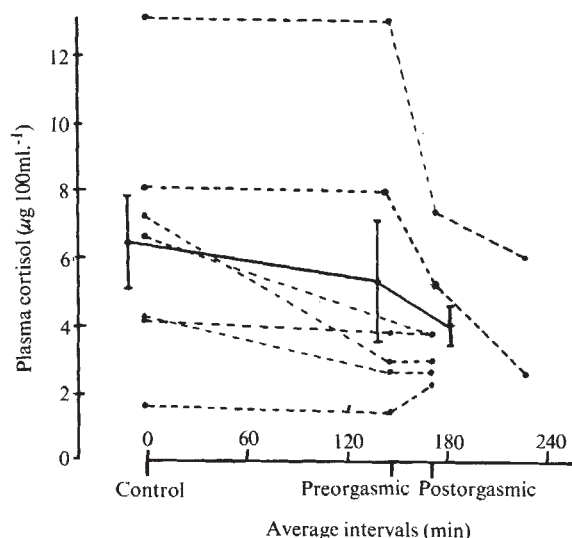


Fig. 1 Relationship between coitus and plasma cortisol concentrations. ----, Control, pre and post-orgasmic samples; —, mean of each group of samples; , s.e.m.

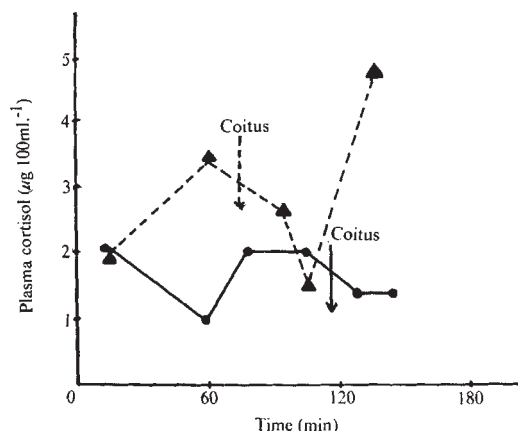


Fig. 2 Acute variations in plasma cortisol levels over a period of approximately 2.5 h during which coitus took place.

between the collection of pre and post-orgasmic samples was 24 ± 2 min (mean $\pm$ s.e.m.).

Cortisol levels in pre-orgasmic samples were similar to or even slightly lower than those of corresponding controls taken nearest to coitus, differences ranging from 0 to $-4.2 \mu\text{g } 100 \text{ ml}^{-1}$ (mean = -1.07 ± 0.67 s.e.m., $n=6$). Levels in the post-orgasmic samples also tended to be lower than in the controls; here differences ranged from $+0.8$ to $-5.8 \mu\text{g } 100 \text{ ml}^{-1}$ (mean = -2.4 ± 0.77 (s.e.m.), $n=7$). On two occasions, additional samples were taken some 40 min after the post-orgasmic specimen; in both of these a further decline in cortisol readings was noted (see Fig. 1). Significant differences between coital samples (either pre or post-orgasmic) and their corresponding controls were not found; nor was there a significant difference between specimens taken before or after orgasm.

The bulk of circulating cortisol is strongly bound to cortisol-binding globulin (CBG) and as a result its metabolic clearance rate is relatively slow. This is reflected by the biological half-life of the hormone which is approximately 70 min². Accordingly, a significant fall in blood cortisol levels—but not a rise—would be difficult to detect if, as in our experiment, the period of observation is relatively short.

Acute changes in cortisol levels measured during two evenings are shown in Fig. 2. On both occasions coitus was associated with a fall in cortisol levels, although on one occasion readings rose again about 1 h after this event.

This study suggests that plasma cortisol levels are little affected by the act of coitus. This is in contrast to the increase noted in other parameters such as blood pressure, cardiac output and respiratory rate^{3,4}. It is also in contrast to the findings with respect to plasma testosterone, levels of which have recently been shown to rise in association with sexual intercourse⁵. Current evidence suggests that throughout the hours of sleep and wakefulness cortisol is secreted in episodic bursts which are separated by periods of so-called quiescence². Our data obtained would be compatible with the view that sexual intercourse was associated with quiescence of this gland. If this turns out to be the case—and studies in other subjects are obviously necessary before definite conclusions can be drawn—then the role of the adrenal cortex in relation to male potency and its disorders requires to be carefully evaluated.

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Seasonal Variation in Twin Births

THE seasonal patterns of single births are well established. Among the birth characteristics noted to be associated with significant changes in seasonal patterns are race, geographic differences¹, and birth order². Early studies on this general topic were reviewed by Huntington³, and other studies by Bailar and Gurian⁴.

Distinct seasonal variability in plural births was noted by Edwards⁵ using data extracted from British records of medical officers of health. He found two distinct peaks (February and September) in the ratio of multiple to single births in his analysis. We report now a study of live born single and twin maternities recorded on the birth certificates of infants born to residents of New York State (excluding New York City) during 1960–1967. A pair of twins was considered a live born maternity if both members of the pair were born alive. This series yielded 1,431,540 single and 14,142 twin maternities (proportion of twins = 0.00978), and is used to investigate the seasonal variation in twinning. Table 1 shows the numbers of twin and non-plural maternities for each month and the relative ratios. The relative ratio for a specific month (r_i) is defined as:

$$r_i = \frac{B_i D_i}{(1/12) \sum_{i=1}^{12} B_i D_i}$$

where B_i is the number of maternities and D_i is the number of days in month i . The comparison of the relative ratios (Fig. 1) shows that the twin births follow much the same seasonal pattern as the non-plural births. The Brandt-Snedecor form of the χ^2 statistic with 11 degrees of freedom⁶ was used to test the null hypothesis that the proportion of twin maternities each month is constant. The calculated χ^2 is 7.52. The probability of observing a χ^2 value greater than 7.52 under the null hypothesis is 0.76. It is reasonable to assume therefore that the seasonal pattern observed in these data in twin births is essentially the same as that observed in non-plural births.

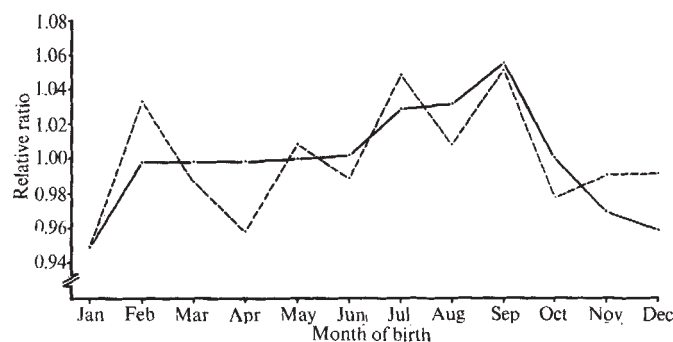


Fig. 1 The relative ratios of non-plural (—) and twin (---) live births by month from upstate New York 1960–1967.

Exact comparisons to Edwards's⁵ findings are not possible since his data were not presented in direct form. In general, however, two points seem clear; his seasonal variability was approximately three times greater than that found in New York State and the pattern of seasonal variability was notably

Table 1 Numbers of Single and Twin Live Maternities and Relative Ratios from New York State Data 1960-1967

	Live maternities			Relative ratio	
	Single	Twin	Total	Single	Twin
January	115,361	1,139	116,500	0.9488	0.9481
February	109,736	1,122	110,858	0.9992	1.0339
March	121,547	1,186	122,733	0.9996	0.9872
April	117,615	1,114	118,729	0.9995	0.9581
May	121,638	1,212	122,850	1.0004	1.0089
June	117,988	1,151	119,139	1.0027	0.9897
July	125,129	1,260	126,389	1.0291	1.0488
August	125,569	1,211	126,780	1.0327	1.0080
September	124,441	1,225	125,666	1.0575	1.0538
October	121,798	1,176	122,974	1.0017	0.9789
November	114,100	1,153	115,253	0.9697	0.9914
December	116,618	1,193	117,811	0.9591	0.9932
Total	1,431,540	14,142	1,445,682	12.0000	12.0000

different. This apparent lack of similarity can result from differences in such factors as sample size; selection; changes in maternal health during the past thirty years; or the composition of the New York and British populations. This latter point may be worth investigation since differences between the seasonal pattern of all live births do exist between the United States and Britain¹.

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Possible Mechanism for the Genetic Component of Language

ALTHOUGH common principles underlie the structure of all languages, they differ in their spoken form by virtue of the diversity in the sounds used. There is no explanation so far for the differences in the sound complement of various languages and the regional dialects, or for the changes in pronunciation which occur over a period of time.

A hypothesis¹, proposed to explain the development of sound complements relating phonetic differences in spoken language to population differences, suggested that a community selects a set of sounds for its communicatory needs in accordance with certain biological determinants. The sound complement is thus an expression of the average preference for those sounds which most people find easiest to use.

Evidence that genetic factors may play a part was derived from the finding that the distribution of the dental fricative (TH) in Europe corresponds to the gene frequency distribution for blood group O. Furthermore, the present distribution of the (TH) sound in the languages of Europe and Asia, and its change during the past 3,000 yr, agree with evidence of human migrations and conquests. Further evidence was subsequently obtained from detailed comparisons of the sounds of language

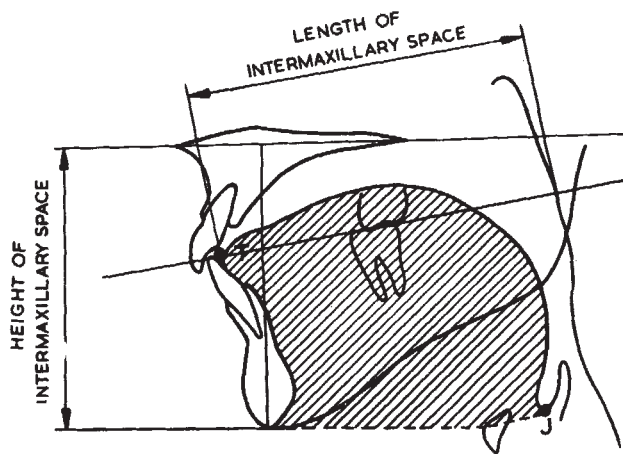


Fig. 1 Intermaxillary space and tongue dimensions from the tracing of a lateral skull radiograph.

and their geographical distribution², and significant racial variations in the structure of the organs of articulation were noted^{3,4}.

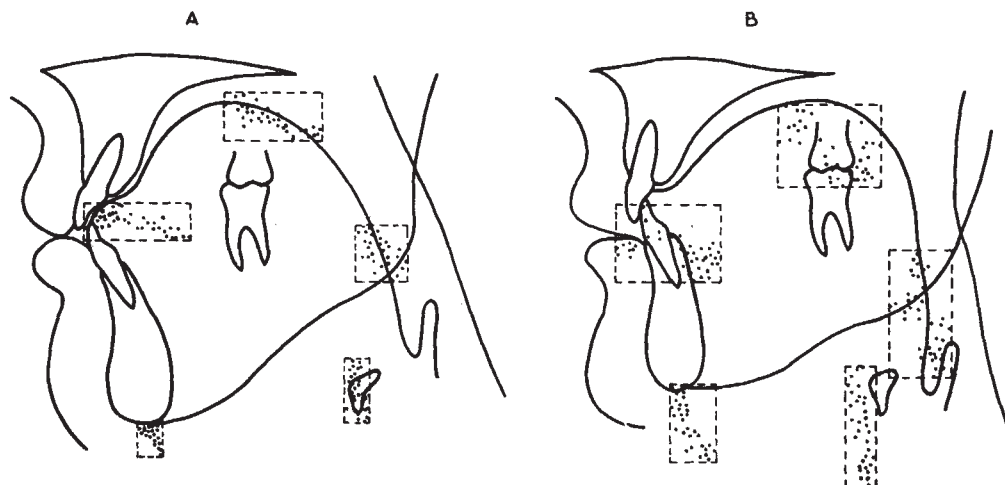
Further evidence came from orthodontic observations on the diversity of human cranio-facial morphology, which showed that genetic variation in the structure and function of the oro-facial complex was the underlying factor in irregularities in the arrangement of the teeth^{5,6}. Variations in structure were consistently associated with specific patterns of posture and behaviour in respiration and deglutition. Clinical studies of orthodontic patients indicated that although speech is a learned motor skill, it is developed by adaptation of inherited patterns of oro-facial behaviour. It was suggested that in any community the pattern of oro-facial behaviour for the production of acceptable speech can be varied⁷ and that these variations can be directly related to inherited variations in the structure of the oro-facial complex.

Although the convergence of these ideas from genetics, linguistics, orthodontics and speech therapy seemed to support the idea of a genetic component in language, no experimental evidence was available to support this theory or to shed further light on the actual mechanism which relates variations in speakers and their speech. My studies have been concerned with establishing both quantitative and qualitative data on the variations in oro-facial morphology and function with particular reference to speech.

Using cephalometrically orientated lateral skull radiographs and high speed cineradiography with image intensification, it has been possible to measure morphological features and also to record patterns of articulatory behaviour of the speech organs. A particular problem arising from the use of high speed cineradiography is the large volume of material which it yields. The problem of quantitative analysis was overcome and a method has been devised for the objective assessment of variations in the pattern of speech movements⁸. In this study the movements of the mandible, hyoid and tongue were analysed. From a study of normal adult male subjects⁹ the following conclusions were drawn.

The pattern of articulatory behaviour is consistent for the individual, but a wide range of inter-individual variation is compatible with the production of acceptable speech sounds. The dimensions of height and length of the intermaxillary space (Fig. 1) (the rigid confines of the tongue) are significantly correlated with the pattern of speech movements. In general, a short and low intermaxillary space is associated with greater movement, and long and high intermaxillary space with less movement. The general direction of movements also differs, being more in the vertical for the former and horizontal in the latter morphological type (Fig. 2). Both tongue size and the size of the intermaxillary space are genetically determined but vary independently.

Fig. 2 Two subjects (A and B) showing variation in the range of movements of the mandible, tongue and hyoid in speech as determined by analysis of high speed cineradiographic records.



Further work suggests that these relationships between structure and function become more obvious if movement patterns are related not only to intermaxillary space size, but also to the available space in which the tongue performs its adaptive functions for the production of speech. Such differences in the amount and direction of movements used for speaking suggest that individuals vary in the total effort necessary for the production of acceptable speech sounds. Variations in the total expenditure of effort associated with differing amounts and directions of movement will affect not only the ease with which a particular sound is produced, but will obviously condition the facility for the production of certain sequences of movement for the production of continuous or connected speech.

The presence of phonemes such as dental or pharyngeal fricatives varies in the languages of man. Likewise, certain sequences of sounds occur in the speech of some populations but not in others. It is therefore probable that population differences in genotype are manifested in characteristic frequency distributions of intermaxillary space dimensions which determine the sounds and their sequential organization in language. Changes in populations affecting their gene-pool will tend to produce variations in the distribution of genotypes. Through such changes the sounds of language may alter to accommodate the articulatory preference of the changing community.

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Gravels, and (2) Maglio's provisional correlation³ of fossil elephants from the "Vaal River gravels" with Kanapoi species "C", radiometrically dated at 4.0 m.y. Wolpoff assumed (ref. 1, page 579) that Maglio's ambiguous term "Vaal River gravels" referred to "the beginning of the Vaal River sequence (oldest gravels)", and hence assumed a correlative date of 4.0 m.y. for the Sterkfontein Faunal Span and associated australopithecines.

The palaeontological literature for the Vaal River^{4,5} reveals that the elephant-bearing deposits are assigned to the Younger Gravels which are Middle Pleistocene in age. Furthermore, Maglio (ref. 3, page 331) writes: "The type and only specimen of each Vaal River species is insufficient for adequate specific definition". Hence, there is no evidence that the Vaal River fossil elephants are conspecific with Kanapoi species "C" or that they have a date of 4.0 m.y.

Mason notes (ref. 6, page 40) that "the Basal Older Gravels and Older Gravels lack animal or human fossils". Without fossil material, no correlations between these two gravel formations and the South African cave sites are possible. Cooke⁷ apparently no longer feels that a Basal Older Gravels-Sterkfontein Faunal Span correlation is valid; it does not appear on his latest chronology chart. He still, however, equates the Sterkfontein Faunal Span with Olduvai Bed I at about 1.9-1.25 m.y.

In sum, the fossil elephants do not assist the dating of South African australopithecines because (1) the Younger Gravels from which the elephants in question were derived are Middle Pleistocene in age; and (2) the Basal Older Gravels are sterile of elephants or other fauna which might be used to correlate this deposit with the Sterkfontein Faunal Span. Furthermore, australopithecine sites in South Africa lack associated elephant material.

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Do Fossil Elephants date the South African Australopithecines?

WOLPOFF has recently proposed a date of 4.0 m.y. for the South African australopithecines¹. He used, however, two stratigraphic correlations which cannot be justified: (1) Cooke's original correlation² of Sterkfontein, Makapansgat and Taung (=Sterkfontein Faunal Span) with the Vaal Basal Older

Mammalian Remains from the Isimila Prehistoric Site, Tanzania

HOWELL *et al.* have reported¹ the results of uranium-series dating of bone from the Isimila prehistoric site, Tanzania. Preliminary identifications of fragmentary mammalian remains recovered during the first excavations at the site (1957–58) were reported in the interim report on the site². These specimens, which are now housed in the National Museum Centre for Pre-history and Palaeontology, Nairobi (but will in future be preserved in the National Museum, Dar-es-Salaam), are discussed more fully here.

(1) Elephantidae (V. J. M.). The eighteen partial plates of elephant molars are very thin, with folded enamel and an index of hypsodonty in excess of 150. These characteristics exclude *Loxodonta* and suggest a rather progressive stage of *Elephas recki*. It might have resembled the Olduvai Bed IV or Ologesailie stage of that species, although it is difficult to be precise on such fragmentary evidence. Because of the lack of traceable *E. recki* material into the later Pleistocene, it is impossible, however, to give an upper age limit. The Isimila material does look more primitive than the terminal stage of the lineage, *Elephas iolensis*, of late Pleistocene age.

(2) Rhinocerotidae (D. A. H.). One fragmentary tooth, comprising the posterior portion of a worn right lower molar, is represented. It is referred to *Diceros bicornis*.

(3) Equidae (D. A. H.). Remains include a proximal metapodial, two teeth and some tooth fragments. Two molars suggest *Equus cf. burchelli*. An upper molar, presumably right M¹, 29 mm long and 30 mm wide, has concave ectoloph halves; the protocone is not markedly reduced anteriorly but not bilobate; there is a distinct pli prefossette, and a small pli caballine. An incomplete lower molar exhibits the flattened outer wall of the hypoconid which is characteristic of *E. burchelli*. The material is not sufficient for definite identification, but these remains cannot be distinguished from those of the bontequagga and are therefore referred to *Equus cf. burchelli* Gray.

(4) Hippopotamidae (S. C. C.). The remains are generally fragmentary and consist largely of isolated teeth (ten) or fragments of teeth (thirty-three). Exceptions are a portion of right maxilla with M^{2/3} and a portion of right mandible with M_{2/3}. These last specimens were part of an individual skeleton, including ribs and vertebrae but lacking extremities or the skull, in a clay underlying Sands 4 in the H.20 area of the site². There is no evidence for the presence of *H. gorgops* as suggested by Howell *et al.*². From the morphology and size of the teeth it is impossible to separate these remains from the living species, *Hippopotamus amphibius*.

(5) Suidae (J. M. H.; S. C. C.). This family is poorly represented. The extinct phacochoerine *Afrochoerus* is known from a left upper and a right lower tusk. The upper tusk is longer and flatter than that of *Metridiochoerus andrewsi* and the wear facet is closer to the base of the tusk. The torsion is less than in *Afrochoerus nicoli* from Olduvai. *Mesochoerus* (cf. *olduvaiensis*?) is represented by an incomplete right M₃, a right P₂, and a right I¹. Postcranial remains include the left and right astragalus, the distal portion of a metacarpal and the distal portion of a right humerus of a pig of similar size to the extant giant forest hog, plus the condyles of a right femur and an atlas vertebra of a somewhat larger suid.

(6) Bovidae (A. W. G.). Remains are infrequent and are generally poorly preserved. Of indeterminate affinity are a tooth fragment, first and second phalanges, immature first phalanx, several molar fragments and a partial left mandible. An incomplete right lower molar is assignable to Bovini, species indeterminate. A right upper molar is assignable to Antilopini, species indeterminate. Alcelaphini are represented by at least two species. *Parmularius angusticornis* (Schwarz), a species hitherto included in *Damaliscus*, is represented by a fairly complete cranium, with partial horn cores. A distal left humerus and a proximal right radius represent an alcelaphine

of this same size. *P. angusticornis*, which at Olduvai Gorge, Tanzania, is temporally restricted to the middle and upper levels of Bed II, also occurs at Laetoli and Peninj in Tanzania and at Kanjera in Kenya. Two other teeth, an unworn right upper molar and a scarcely fossilized lower molar, also represent an alcelaphine, of indeterminate species. A left proximal radius is also alcelaphine, but is larger than *P. angusticornis* and probably represents another species. An isolated lower molar represents a small bovid, either Neotragini or Cephalophini. Altogether at least five species of four tribes of Bovidae are represented in this collection.

(7) Leporidae (J. M. H.). This family is represented by a single left mandible fragment with P₄–M₁. The fragment includes that portion from the posterior edge of the P₃ alveolus to the anterior half of the M₂ alveolus. The occlusal surfaces of the teeth are badly weathered, making identification even to generic level unreliable. The teeth are distinctly smaller than those of *Lepus veter*, and slightly smaller than those of the extant *L. capensis*.

In addition to these fossil specimens, all of which derive from Sands 4, the collection contains a facial region, with left I to M³ and right I to M¹, of a rodent referred by one of us (J. M. H.) to the giant rat, *Cricetomys gambianus*. This specimen was recovered from Sands 1 in the K-14 area of the site. The bone seems to be recent and because the giant rat is a burrowing rodent it seems likely that it found its way into the Pleistocene sediments quite recently.

The mammalian taxa identified at the Isimila site still do not permit a much finer resolution of the age of the fossiliferous sediments. The elephant, suid and hippo remains suggest an age comparable with some part of Olduvai Bed IV and the Ologesailie succession. The equid remains are consistent with that interpretation. The only possible conflicting evidence is the occurrence of the extinct alcelaphine, *Parmularius angusticornis*, which at Olduvai is found only in the mid and upper parts of Bed II, but it is possible that this species continued to exist over a still more substantial time span.

There is now palaeomagnetic evidence³ which suggests that the Matuyama–Brunhes boundary, which has an age of ~0.7 m.y.⁴, occurs at or near the Olduvai Bed III/IV junction. If that is the case, then the results of the Uranium-series dating of Isimila bone, yielding values of >170,000 and ~260,000 yr, is indeed quite reasonable.

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BOOK REVIEWS

Preventive Medicine and the Environment

Environmental Health. Edited by P. Walton Purdom. Pp. xiv + 584. (Academic: New York and London, September 1971.) \$19.50; £9.10.

Epidemiology as Medical Ecology. By W. Le Riche and J. Milner. Pp. ix + 460. (Churchill Livingstone: Edinburgh and London, 1971.) £6.

Just Consequences. Edited by Robert Waller. (A Study of Contemporary Health Problems in Relation to Nutrition which suggests that there is a New Pattern of Diseases in Western Civilization.) (Charles Knight: London, November 1971.) £2.50.

Marine Ecology: Environmental Factors, Vol. 1, Part 2. Edited by O. Vinne. Pp. 576. (Wiley-Interscience: London, 1971.)

THESE four books all have a bearing on the continuing debate on ecology. Three of them in their differing ways represent contributions from the sciences and technologies which together make up preventive medicine. The fourth has less direct relevance in this context, but because of its subject matter—the ecology of the oceans—it covers important areas of ecological discussion and in many ways is the most satisfactory of the four publications.

By now we are all too familiar with the noise of the rattling bones of the main arguments put forward by the doom and gloom school. Coming later upon the scene, but, it is hoped, not too late to still some of the incipient panic, come counter-arguments from calmer voices. The heat and noise generated by this debate, the emotionalism displayed by the more anxious of the ecologists, the over-ready acceptance by progressive people for the "need to do something", could make the appearance of sober works such as these a welcome event. Any assemblage of agreed facts and established data, particularly if they relate to human life, health and behaviour, should contribute to objectivity and rationality. This in turn may lead to calmer decision-making and perhaps to fewer hasty actions based upon inadequate knowledge. The titles are, alas, seductively deceptive. "Ecology" and "environmental health" are fashionable terms of the moment and do not in fact adequately reflect the contents of the volumes.

The first two books are too short to deal adequately with their subject matter—but here is the dilemma of attempting to deal in single books with vast interlocking areas of knowledge. What to leave out becomes crucial and yet how to maintain a judicious balance and still present unbiased statements is a dilemma not solved here. No such questions of balance, however, seem to have troubled the majority of contributors to *Just Consequences*.

Environmental Health is a curious amalgam of the simple and the complex, the naive and the profound, and the precise audience for which it is intended is far from clear. To English eyes it appears to be a do-it-yourself manual for intelligent young college graduates about to set up house in a new suburb of an American city which is desperately deprived of community services and dependent on the initiative of individuals and groups to provide for their own needs. There are ten chapters covering a comprehensive range of environmental hazards from insects and vermin to sewage and noise. Within each chapter the individual authors take great leaps from elemental descriptions of the hazards to detailed nomograms and technical drawings useful in their control. Not enough is said for the novice on some topics, and too little is said on others to make it a textbook. It can be imagined to be useful in the library of a secondary school as an aid to teachers and students grappling with topic teaching on pollution. References to work done in the last 3–5 years are curiously lacking in many places.

The book by Le Riche and Milner is deeper in its intentions. The trendy use of "ecology" is deplorable as this is essentially a primer in epidemiology. There is no need to give it this extra puff as it is a good enough text of its kind without it. The publishers state that this is not a textbook, and this is true; it is probably impossible to write an adequate standard textbook on epidemiology in its present state of rapid expansion. It sets out very adequately the established range of epidemiological techniques. There is a major flaw from the British point of view—it pays disproportionate attention to classic microbiological epidemiology and the same defects appear in other North

American works on the subject. On the other hand, it does set out clearly the essentially simple parameters of host, agent, time and place, with working examples. It gives less than adequate attention to non-communicable diseases but gives a passing but interesting glance to possible future fields of work on malnutrition, dentistry, psycho-social illness, and the family. References are adequate and fairly full.

Alas, such is the nature of epidemiology that chance associations of observations can be uncritically accepted as suggesting cause and effect, the more so if the author's prior speculations about causation seem to be supported by these inductions. Le Riche and Milner show strange reluctance to accept the evidence supporting the protective effects upon human teeth of one ppm of fluoride when added to public water supplies, in spite of the wealth of detail they set out in its support. On the other hand, they report the pathological effects of fluoride on bones and joints which are caused by much higher concentrations without stating the rarity with which they occur. There is also a somewhat uncritical inclusion of the theories of Cleave and others on the "Saccharin Disease" which is introduced as a "fascinating general epidemiological theory". It is to be hoped that epidemiology will in time produce general theories, but this moment does not seem to have arrived. Whether this is so or not can probably be judged by reading Cleave's own synopsis of his hypothesis in *Just Consequences*.

This work is a series of contributions from different authors describing the effects on health of modern dietary habits and food production. The essays largely support the validity of their cases by the presentation of highly selected evidence to prove various prior assumptions. Not surprisingly, it is a much shorter work than the others. The threats to health are over-refined foods, the presence of food-additives, unnatural methods of food production and modern methods of soil cultivation. The theories behind many of the viewpoints are explained in detail, often supported with more enthusiasm than evidence. Here the inductive process inherent in epidemiological method runs

amok. Indeed, it is to be hoped that for the future health of mankind some of these hypotheses will be proven, and the control of some intractable diseases can begin. This work does little to help this task. It is of passing interest that some of the authors rely on personal evidence from overseas territories with different cultures and dietary habits from our own. Perhaps it is here rather than in Britain that the missing evidence may be found.

The virtues of brown bread, unrefined carbohydrates, unsaturated fatty acids and soil conservation by "natural" methods are all laid out. References are scanty and in some contributions are non-existent. There is an interesting and balanced contribution from Mellanby on "The poisons in our food". The work is a good example of all that is worrying about the stridency of some of the partisan arguments that too often cloud and divert many serious ecological discussions.

Marine Ecology is a different work altogether. This book is Part 2 of Volume 1 in a series of five, covering the subject in great detail. It aims, when complete, to be "a comprehensive integrated treatise on life in oceans and coastal waters". This book covers the salinity, water movement and turbidity of the oceanic environment. It is a work of reference, not a textbook and not a tract. Meticulous care has produced a handsome, balanced, objective, profoundly referenced and accurately indexed work. Naturally, at times it makes tedious reading; it is global in its scope and cautious in its statements. If this book is representative of the whole treatise, it deserves, when complete, to become the standard work on the subject.

Thinking more broadly in the context of the control of the whole living environment, there are odd important omissions when these four books are considered as a group. Little is said about the main obstacle of environmental control, and this is the appalling difficulty of changing patterns of human behaviour. This is probably the single major obstacle to advances in improving our milieu. For example, if we are to control the population, we must seek a deeper understanding of the human motives behind the limitation of family size. We cannot idly rest upon the presumption that rising standards of living in developing countries will inherently restrain population growth in some unknown, albeit well-documented manner. Although this phenomenon did occur in Europe and North America during the last 50 years it is doubtful if we can afford to wait passively and see if it happens again in other quite different countries. Indeed, if it does not happen and as we do not understand why it happened in the first place, what then?

If the technology of contraception were as widely applied as that of plant genetics we should be much less troubled. More research is needed into understanding human motivation in this area; much more than even the large sums being devoted to the search for a perfect contraceptive.

Yet again, little is said about gaining the recognition by the broad mass of the population that any environmental problems exist at all. This is vital. Within the limited circle of the knowledgeable, people continue to disagree about size and time scales, but there is a general acceptance that the air, water supplies, land resources, the oceans and so on, all need protection. But protection ultimately means the restriction of personal liberty and a personal willingness to spend money which will be diverted from other, perhaps more pleasurable, expenditure. Neither of these consequences has been generally accepted nor probably even realized by the mass of the public. There is some evidence to show that this is so. Clean air is the great success of environmental control in the last decade, but the costs of introducing smokeless zones had to be disguised from taxpayers and ratepayers as "compensation" for the installation of new appliances. Water authorities are increasingly hamstrung by public resistance to increases in water rates, which are largely nominal related to the capital needs required for conservation. At present, the public is accustomed to acquiesce to a general level of expenditure which is minute compared with the most limited estimates of the future costs of conservation. In 1972 we have a long way to go before *The Times* will agonize again, as it did in 1872, about the threatened halt to industrial expansion caused by the diversion of men and resources into massive water supply and sewage works.

Environmental control has its present base of concern largely in the middle class, who are often defending a limited number of precise vested interests and whose commitment can rapidly fade once the immediate battle is won. A big public education campaign, which must be properly prepared by adequate prior research, will be needed before the electorate will provide the politicians with the will to find adequate money for the work ahead.

The content of *Environmental Health* illustrates another disturbing gap in our resources: how little scientific effort has been invested in the recent past in the search for more efficient control systems. Dust control measures still largely depend today on simple engineering methods invented before the turn of the century. Only one fundamental new method of food preservation, namely ionizing radiation, has emerged in the present century, and this is only

of limited application. All the others, such as freeze drying, are simply combinations of pre-existing knowledge. The last generally useful advance in the purification of water was the introduction of chlorination in 1907. Sewage is still most frequently dealt with, slowly but efficiently, by the trickling filtration process through bacteria beds, introduced in the late nineteenth century. Even today in the USA over 50 per cent of sewage is still subject to treatment by even older methods, namely simple sedimentation, as in a septic tank. Electrostatic precipitation, the most modern method of controlling air pollution, was introduced in 1907. These facts make an ironic contribution to the present debate on the organization of research.

Finally, any attempt to control our living world must aim at educating the public to come to terms with the application of science and technology to our daily lives. Somehow the technological imperative must be controlled. We must all come to realize that each new application of technology to the "progress" of our high-consumption society does not have to be blindly accepted as an inevitable or even necessary addition to our way of life. For example, it is difficult to see what benefits accrue to health even in its broadest sense from the introduction of colour television in its present imperfect form. Yet eventually we shall have to dispose safely and perhaps even usefully of the remnants of all those monochrome receivers which were so very satisfying to their owners until only a few years ago. We have produced another and unnecessary burden on our resources and yet another problem of pollution. As our society is organized at present, with so many pressures encouraging such "advances", it is difficult to see such restraint coming about. If it did, even in the smallest way, the anxieties would be fewer and the tasks less onerous. W. T. JONES

The Columbidae

Pigeons and Doves of the World. By Derek Goodwin. Pp. v+446+3 plates. (British Museum of Natural History: London, 1971.) £7.

A KNOWLEDGE of the systematics and adaptive radiation of any major species group is of considerable value to the ecologist, ethologist and those others concerned with comparative biology. The authorities of the British Museum of Natural History are, therefore, to be congratulated on commissioning one of their experts to deal with the Columbidae in a lavish and well produced book, which now appears as a second edition. There is an account of all existing and recently extinct species,

separate paragraphs covering description, field characters, world distribution (with maps), habitat, feeding and breeding habits, voice and displays. For most species there is a line drawing by Robert Gillmor, these being based on rough sketches by the author who is himself a gifted illustrator. Of course, colour illustrations would have been better but costs prevented this and so there are only three colour plates depicting family diversity, sexual dimorphism, and adult and juvenile plumages in certain species where considerable differences exist. For many of the species there is still a marked paucity of field knowledge and inevitably the ecological sections are brief and the feeding habits summarized by such statements as: "feeds both in branches and on ground on seeds and berries". Fortunately many of the pigeons have been kept in captivity and there are observations, albeit sometimes anecdotal, by aviculturalists to supplement some of the sections dealing with behaviour.

So much literature has been reviewed that it is a pity that more references have not been reproduced; bibliographical details must be one of the main functions of a book of this kind. This was a serious criticism of the first edition and about thirty new citations have now been given. But it remains true that there are gaps and the book cannot be relied on to provide an up-to-date entry into Columbidae literature. For example, since the first edition reasonably important papers have appeared dealing with the feeding and breeding ecology of the rock dove *Columba livia* and turtle dove *Streptopelia turtur* and these are omitted. The author has now added a previously neglected paper on the breeding biology of the band-tailed pigeon *C. fasciata* but fails to record an important one dealing with mortality estimates for this species which appeared in 1967; in view of the paucity of data for most non-European pigeons these omissions cannot really be afforded.

Separate chapters are devoted to topics such as nomenclature, plumage, feeding and breeding habits, and there are six chapters which reflect the author's interest in ethology. These chapters add to the interest of the book and like the main text are interjected with the author's own experiences and observations which are stimulating to read. But they suffer from being a rather one-sided assessment which is not appropriate to a general treatment of this kind. For example, there is an enormous literature on pair formation in *Streptopelia risoria* and on the endocrine-behavioural correlates associated with egg-laying and incubation behaviour; yet no mention of D. S. Lehrman and his colleagues at Rutgers

University appears in the relevant sections. Such casual statements as "the wood-pigeon maintains its numbers in spite of persecution" are acceptable even if relatively meaningless. But it is not acceptable to add the highly contentious suggestion "that this is probably consequent on game preservation reducing its natural enemies", in contradiction to intensive scientific studies of the bird which show this sort of conceptual approach to be incorrect and outmoded. It is a pity that some of these points were not dealt with when the second edition was prepared but in spite of them the book remains a very worthwhile contribution to ornithology, containing as it does so many first-hand accurate and original observations by a highly perceptive worker. The radiation of the Columbidae is really excellently portrayed in punctilious detail; the minute white spot on the wing of the purple quail dove is intentional and not a block-maker's fault as the non-initiated may suppose.

R. K. MURTON

The Locust Problem

The Desert Locust. By Stanley Baron. Pp. xiv+228+16 photographs. (Eyre Methuen: London, January 1972.) £3.50.

FROM the Eighth Plague of Egypt to the present day the desert locust has intermittently ravaged an area of 25 million square miles. Mr Baron has written an excellent account of the biology of this major and persistent insect pest, and the measures taken to subdue the 1967-69 plague. This book is for the general reader, and the insect physiologist will find some oversimplifications, such as the description of the cuticle as a "box containing the creature's physical and nervous systems"; and some general insect features are attributed specifically to the locust. These, however, are minor criticisms of a book intended to convey the complexity of the locust problem to the non-specialist. Britain has always been prominent in locust research and control, mainly because of the extent of our late Empire and the untiring work of the late Sir Boris Uvarov. The author describes the prolonged battle to put locust control on an international basis, and to expand basic locust research. Insect control has often been held back by a lack of basic information, and this is illustrated by the description of the early concept of the "inhalation" of poisonous dust sprays by locusts. One of the most interesting aspects of locust biology is phase change. The solitary phase of the locust differs in morphology, physi-

ology, and behaviour from the swarm-forming gregarious phase. This book does not elaborate on the fascinating problems of developmental biology involved in phase change, but concentrates more on the ecological aspects, for example, the evidence that swarm outbreaks are caused by the crowding of locusts in limited areas of vegetation, followed by multiplication and gregarization. Much of the book is based on the author's experiences in the field with locust control officers. Locust swarms are controlled largely by chlorinated hydrocarbon insecticides, so raising a conflict between control and conservation. When forty-five goats were killed by insecticide poisoning, the author agreed with the verdict of the Governor of Dankalia that this was justified to kill locusts, which illustrates that in many underdeveloped countries the nebulous long term disadvantage of insecticide use is, when set against the possibility of immediate starvation, a western sophistry. This book shows that international cooperation is possible in the face of an international threat, and that large areas of the world depend on extensive insect control measures which, until further research is done, must inevitably be broad spectrum chemical methods.

L. HILL

Development of the Dog

Integrative Development of Brain and Behaviour in the Dog. By Michael W. Fox. Pp. xii+348. (The University of Chicago: Chicago and London, October 1971.) £10.15.

THE purposes of this book are, first, to integrate recent neurological and behavioural theorizing and research relevant to the problem of development; and secondly to present some original multidisciplinary data gathered by the author specifically on the development of the dog. Of its seven chapters, the first is concerned with a critical review of some of the more salient issues in the field. In chapters 2-6, Fox presents a series of original studies concerned with the development of reflexes, electrical brain potentials, central anatomical and biochemical changes, simple and complex types of learning and the effects of various kinds of early experience. In the final chapter and epilogue, an attempt is made to provide a comprehensive theoretical framework for the future study of development based on the key concept of "integration".

It cannot be said that the book accomplishes its rather ambitious goals. The literature survey strikes me as having a "grab-bag" sort of character—many surprises both in its inclusions and in

its omissions. Similarly the presentation of original studies composing the bulk of the book is uneven and often seriously lacking in detail. They might more suitably have been submitted in conventional format to standard journals. Finally, the attempt at educing a grand theoretical design based on the concept of integration is interesting but does not offer any very useful guidelines to the researcher in the area of development.

These, however, are general criticisms. The book contains many good specific insights and brings together some diverse and little-known material. The latter feature reflects the broad background of experience of the author and his professional associations with such figures as J. P. Scott, Williamina Himwich, J. Myslivecek, R. L. Sidman and J. L. Fuller. Nor can one underestimate the amount of effort put into some of the studies and the value of some of the basic data presented. Developmental psychologists should find the book a useful addition to their libraries.

W. R. THOMPSON

Undergraduate Mineralogy

Mineralogy for Students. By M. H. Battey. Pp. xii+323. (Oliver and Boyd: Edinburgh, January 1972.) £4.

THIS book aims to provide a unified treatment of the principles of mineralogy combined with descriptions of the commoner minerals in a form that is suitable for first degree courses in geology or mining.

In the first of its two parts, "Principles and Methods," crystal chemistry, geometrical crystallography, the physical properties of crystals (with an understandable emphasis on optics), X-ray crystallography and mineral associations are among the topics discussed. Inevitably, exploration of such a wide range of subjects in only just over half the text means that the treatments must in places be rather superficial; nevertheless, the presentation is coherent and informative, and is supplemented by suggestions for further reading which one hopes the student will pursue. In the second part, "Descriptions of Minerals", the structural features, crystallographic constants, habit, optics, occurrences and so forth of the commoner rock-forming minerals are set out systematically. Such data are basic to books of this kind, but in this volume are clearly and succinctly presented in a manner that the student will generally appreciate; in particular the structural diagrams are to be commended for the effort to remove confusing detail. The four brief appendices include elementary determinative

tables; one might hope that the historical nomenclature of crystal classes also mentioned here could now be decently buried. Within its brief, the book generally achieves its objectives and stands comparison with the best of overseas texts which have mainly occupied this ground so far. But, even in a first degree course, mineralogical studies cannot be divorced from their context, and it is disappointing to find that the physico-chemical principles essential to a fuller understanding of rock-forming processes are deliberately omitted.

P. GAY

Nicotinic Acid

Metabolic Effects of Nicotinic Acid and its Derivatives. Edited by K. F. Gey and L. A. Carlson. Pp. 1251. (Hans Huber: Bern, Stuttgart and Vienna, 1971.) 345 francs; 310 DM.

THIS book contains the proceedings of a workshop held in Films, Switzerland, in March 1970. The importance of nicotinate (and its derivatives) in pharmacology and therapeutics, and hence the basis for this workshop and these extensive proceedings, lies in its effects in lowering plasma cholesterol and triglyceride concentrations and its potential value in the treatment of atherosclerosis. The book contains some one hundred and twenty papers in all by a number of distinguished scientists in fields of metabolism, metabolic regulation and pharmacology. The editors are well known to all in these fields and they are to be congratulated on the timeliness of the topic, on the coverage given, on the quality of the published proceedings, and on its undoubted worth and value. This book reviews pharmacokinetics of nicotinate and derivatives, metabolism of nicotinamide nucleotides, mobilization and metabolism of fatty acids, including the role of cyclic AMP, the metabolism of triglycerides and the role of lipoprotein lipase, cholesterol metabolism, certain aspects of carbohydrate and protein metabolism and hormonal effects and actions. The unifying theme is aptly described in the title of the book.

It is perhaps inevitable with so many participants that the subject is somewhat fragmented, that some presentations are incomplete, and a number of the papers are extremely short. Nevertheless it is a comprehensive coverage of the physiological role of nicotinate and its pharmacological effects. Quite apart from the bibliography associated with each of the individual papers there are seventy pages of general bibliography in the supplement. The printed discussion of the papers is well

directed and contains some important statements and questions.

This book should form a valuable work of reference for those interested in pharmacology, in cardiovascular disease, in lipid metabolism and in metabolic regulation. The quality of reproduction and editing is quite outstanding. As a specialist work in this field it can be thoroughly recommended.

P. J. RANDLE

Educating Physicists

Teaching Physics—An Insoluble Task? Edited by Sanborn C. Brown, F. J. Kedves and E. J. Wenham. (Proceedings of an International Conference on the Education of Teachers of Physics in Secondary Schools, Hungary, September 1971). Pp. xiv+261. (The MIT Press: Cambridge, Massachusetts, and London, 1971.) \$10.

It isn't only the football manager who has trouble in bringing together the separate skills of distinguished players. Editors of congress proceedings find similar problems and this record of the IUPAP meeting held in Eger in the summer of 1970 is no exception. Certainly the participants included many illustrious names in the field of science education and were led by Kapitza whose opening address was full of his usual wisdom. Nevertheless the invited papers on a wide range of topics from constraints on teacher education to the technology of physics education did not fit smoothly together and give one the understandable impression of individual prima donnas playing together for the first time. The editors attempted, as they say, to bring across through the very highly edited proceedings something of the flavour of the group discussions to those who missed the opportunity of attending the meeting.

Unfortunately, verbatim reports frequently bring in unnecessary redundancies and divergencies and I doubt whether the hoped-for tang comes across well to those of us who missed the international. One must hope that the benefits will be apparent to those who were there although I found it particularly difficult to believe that the discussions which led, for example, to the rather well worn recommendations of the working group for curriculum innovation could have been too novel or exciting. Throughout the text there are some useful biographical lists.

It is easy to be critical of proceedings of this kind, and I am sure that the editors realized that their own task of presenting them was far more difficult than the teaching of physics.

K. W. KEOHANE

CORRESPONDENCE

Imprinting

SIR,—The earliest reported historical references to imprinting frequently cited in reviews of the subject¹⁻³ are the systematic observations of Spalding⁴. Spalding observed that "chickens as soon as they are able to walk will follow any moving object. And, when guided by sight alone, they seem to have no more disposition to follow a hen than to follow a duck, or a human being".

The following response and the formation of attachments of young nidifugous birds were noted, however, more than 350 years earlier by Sir Thomas More. In his classic treatise *Utopia*, first published in Latin in 1516, appeared the following account of a practice in Utopia:

"They breed vast numbers of chickens by a most extraordinary method. Instead of leaving the hens to sit on the eggs, they hatch out dozens at a time applying a steady heat to them—with the result that when the chicks come out of the shells, they regard the poultryman as their mother, and follow him everywhere!"⁵.

One wonders whether More's allusion to imprinting was derived from then-existing knowledge applied perhaps in some foreign land, or was the product of his own observations and imagination. In either case it appears doubtful that remarks presented in a treatise on a utopian state would be addressed to knowledge common to Britain at the time. It would be difficult to assess the heuristic value of More's statements on imprinting for subsequent thinkers, but one is reminded of Chaucer's words (lines 24-25, *The Parlement of Foules*): "And out of olde books, in good feith cometh al this new science that men lere".

Yours faithfully,

MICHAEL D. SHALTER

Department of Zoology,
University of Maryland,
College Park, Maryland

¹ Gray, P. H., *J. Gen. Psychol.*, **68**, 333 (1963).

² Sluckin, W., *Imprinting and Early Learning* (Aldine, Chicago, 1965).

³ Bateson, P. P. G., *Biol. Rev.*, **41**, 177 (1966).

⁴ Spalding, D. A., *Macmillan's Mag.*, **27**, 282 (1873); reprinted in *Brit. J. Anim. Behav.*, **2**, 2 (1954).

⁵ More, T., *Utopia*, 71 (Penguin Books, Baltimore, 1965).

Porphyria

SIR,—In a recent letter (*Nature*, **235**, 388; 1972), Dalton, McAuliffe and Slater presented additional evidence for the well-established observation that protoporphyrin-IX is a singlet oxygen (¹Δg) sensitizer^{1,2} and suggested that β-carotene (a singlet oxygen quencher) might be effective in the treatment of some types of porphyria. I wish to point out that the therapeutic use of β-carotene as a photoprotective agent was proposed by Mathews in 1964 on the basis of studies on porphyric mice³, and that the effectiveness of β-carotene therapy in humans with erythropoietic protoporphyria has already been demonstrated^{4,5}.

Yours faithfully,

ANTONY F. McDONAGH

Department of Medicine,
University of California

¹ Rawls, H. R., and van Santen, R. J., *J. Amer. Oil Chem. Soc.*, **47**, 121 (1970).

² Politzer, R. I., Griffin, G. W., and Laseter, J. L., *Chem. Biol. Interactions*, **3**, 63 (1971).

³ Mathews, M. M., *Nature*, **203**, 1092 (1964).

⁴ Mathews-Roth, M. M., Pathak, M. A., Fitzpatrick, T. B., Harber, L. C., and Kass, E. H., *New Engl. J. Med.*, **282**, 1231 (1970).

⁵ Mathews-Roth, M. M., Pathak, M. A., Fitzpatrick, T. B., Harber, L. C., and Kass, E. H., *Trans. Assoc. Amer. Phys.*, **83**, 176 (1970).

Population Growth

SIR,—Your editorial of March 17¹ includes the possibility, which is already occurring to many people, that the work of Professor Ehrlich and others may conceivably have already "overkilled" the growth of US population, in certain social groups at least. The effects of this trend of opinion are by no means exhausted—an academic friend of mine received hostile correspondence on giving birth to a fourth child. You do not cover another demographic imponderable, the possibility within a reasonable time of fundamental interference with the rate of ageing. This is now a contingency only, but its most likely date of emergence as an option has been put between 1993² and 2023³. Neither of these Delphic predictions appears to allow for the time taken for an experimental method to validate itself actuarially—however, with the development

of short-term measurement of the ageing rate⁴ it might be realized experimentally.

Taking experimental gerontology at its present rate of funding and progress, rather than any science-fiction level, it is still a factor to consider before we become alarmed at the dependency ratio in a naturally-ageing population which is following a policy of replacement only. Although on a plausible scenario the maximum likely gain in world population from artificial slowing of ageing is unlikely to exceed 7% by 2050⁵, gerontologists are often attacked on the ground that their researches are demographically inopportune. I have been accustomed to argue that if we need "zero population growth" they are the reverse, at least in the overconsuming developed countries most likely to apply any new knowledge, on the grounds that delay in ageing would increase the productive portion of the lifespan, and that, as in the case of the elephant compared with the mouse, low reproductive rate is equilibrated with longer life: "zero population growth requires longterm people". None of these possibilities are so far from clinical trial that a 50-year view can afford entirely to overlook them.

Yours faithfully,

ALEX COMFORT

Department of Zoology,
University College London,
Gower Street,
London WC1E 6BT

¹ *Nature*, **236**, 87 (1972).

² Bender, A. D., Strack, A. E., Ebricht, G. W., and Von Haunalter, G., *A Delphic Study of the Future of Medicine* (Smith, Kline and French, Philadelphia, 1969).

³ Gordon, T. J., and Helms, O., *RAND Corp. Rep.*, P-2982 (September 1964).

⁴ Comfort, A., *Lancet*, **ii**, 1411 (1969).

⁵ Prehoda, R. W., *Proc. Conf. Social Implications of Lifespan Change* (Center for the Study of Democratic Institutions, Santa Barbara, 1970).

Knuckle Walking

SIR,—E. N. Tiritsoo (*Nature*, **236**, 472; 1972) notes the misleading word "linemen" in our communication on knuckle-walking. Our manuscript read "linemen", a designation for American football players who crouch in a line with hands on the ground prior to the attack. Unfortunately, on the proofs

we did not detect the "s" which removed them from the turf in Chicago to the sidelines in Buckinghamshire.

Yours faithfully,

RUSSELL TUTTLE

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Department of Anthropology,
1126 East 59th Street,
Chicago, Illinois 60637

Disaster Research

SIR,—The London Technical Group is in the process of compiling a list of people engaged in research which could be of value to workers in the disaster field.

We would be grateful to hear from anyone doing relevant research in such

fields as medicine, nutrition, engineering, sociology and so on.

Yours faithfully,

MARY ANNE BAKER

London Technical Group,
Research Institutes Working Party,
35a Brackenbury Road,
London W6 0BE

Obituary

Professor J. J. Weiss



JOSEPH JOSHUA WEISS, Emeritus Professor of Radiation Chemistry at the University of Newcastle upon Tyne, died on April 9, 1972, at his home in Newcastle.

Born in Austria in 1905, Weiss studied at the Technische Hochschule in Vienna, where he obtained his doctorate. After spending two years (1928–30) at the Textile Institute in Sorau he moved to Germany to become assistant to Fritz Haber at the Kaiser-Wilhelm Institut für Physikalische Chemie und Elektrochemie, Berlin. The short period spent with Haber was a very important one; not only was Weiss's scientific career essentially moulded during this time, but it also provided an opportunity for him to establish personal contact with many of the great figures in the physical and mathematical sciences. A scientific product of this period is the well-known Haber-Weiss mechanism for the catalytic decomposition of hydrogen peroxide by ferrous ions; involving, as it does, the intermediate production of free radicals it is a milestone in the history of studies of free-radical reactions in solution.

The political atmosphere in Germany in the early thirties affected many of the scientific institutions so that in 1933 Haber, accompanied by Weiss (his last assistant), left Berlin to take up a post at the University of Cambridge. After Haber's death, which occurred soon afterwards, Weiss moved to University College, London, carrying out a research programme and obtaining a PhD degree. Ideas on the role of electron-transfer processes in the mechanisms of photochemical and thermal reactions laid the foundation for the research work during the following years. Such processes were postulated to occur in the quenching of fluorescence, in dye-sensitized reactions and in chemiluminescence. Weiss discovered the role of positive hydrocarbon ions in systems containing aromatic molecules and polycyclic hydrocarbons; this particular work led to research on the formation and structure of molecular complexes, which were postulated to be charge-transfer complexes. Weiss's long association with Newcastle upon Tyne began in 1937 with an appointment to a demonstratorship in King's College, University of Durham (now the University of Newcastle upon Tyne). In 1942, Weiss became interested in the theory of chromatography and published the first rather detailed mathematical theory for equilibrium conditions. It was at this stage, however, that he first began his researches into the chemical effects of ionizing radiations, particularly in solutions. As a consequence of the many significant contributions which then followed, Weiss may be rightly described as one of the fathers of the now extensive field of radiation chemistry. Thus, in 1944, he proposed that chemical effects in aqueous solutions can be accounted for by H atoms and OH radicals, this free-radical theory being elaborated in a large number of systems, including solutions of substances of biological interest. The implications of these ideas to radio-

biology were pointed out, allowing a new avenue of approach in this important area. The many productive years which followed led to Weiss's eventual promotion to a professorship and to the establishment of the Laboratory of Radiation Chemistry at Newcastle.

Professor Weiss was one of the pioneers in the field leading to the discovery of the hydrated electron as an entity in irradiated aqueous solutions. He was particularly interested in the exact physical state of trapped electrons and was recently working on this problem again.

On the occasion of his sixtieth birthday, and as a tribute to his many achievements, an International Conference on Radiation Chemistry and Photochemistry was held at the University of Newcastle upon Tyne. International recognition has led to the conferment of a honorary degree by the Technical University of Berlin (1968), the award of the Marie Curie Medal of the Institut du Radium (1970), and, recently, to the establishment of the Weiss Medal by the Association for Radiation Research. On retirement from his chair in Newcastle, an issue of the *International Journal for Radiation Physics and Chemistry* was dedicated to him "in appreciation of his exceptional contributions to radiation and photochemistry". Latterly, Professor Weiss was conducting research programmes at the Max-Planck Institut für Kohlenforschung, Mulheim, and at the Paterson Laboratories, Christie Hospital and Holt Radium Institute, Manchester; such activity in his official retirement was characteristic of his life-long dedication to science.

Professor Weiss will be remembered not only for his research work and his stimulating presence at scientific gatherings, but also, with gratitude, by the many pupils and colleagues who have benefited from his wide-ranging knowledge of science and of scientists.

Announcements

University News

Dr J. W. Black, Smith Kline and French Research Institute, has been appointed to the chair of pharmacology tenable at University College, and **Dr M. J. Perkins**, King's College, has been appointed to the chair of chemistry tenable at Chelsea College, **University of London**.

Barry T. Turner has been appointed to the chair of industrial management, **University of Newcastle upon Tyne**.

Dr R. W. Baldwin has been appointed professor in tumour biology, **University of Nottingham**.

Dr Hans Motz has been appointed to the chair of engineering, **University of Oxford**.

Appointments

Lord Stokes of Leyland has taken office as president of the **Institution of Mechanical Engineers**.

Professor F. R. Bell, Royal Veterinary College, London, **Mr D. W. Jolly**, Huntingdon Research Centre, and **Dr G. E. Paget**, Smith Kline and French Laboratories, have been appointed members of the **Medicines Commission**.

Mr Umberto Colombo, science adviser to the Italian Minister for Research Coordination, has been elected chairman of the **OECD Science and Technological Committee**. **Sir Ronald Walker**, **Professor Frits Botcher** and **Mr David Beckler** were elected vice-chairmen.

William C. Ackerman, Illinois State Water Survey, **Paul F. Chenea**, General Motors Corporation, **W. Kenneth Davis**, Bechtel Corporation, and **W. Denning Lewis**, Lehigh University, have been elected to the Council of the **US National Academy of Engineering**.

Miscellaneous

Sir Rudolph Peters has been awarded the second **British Nutrition Foundation prize**, for his distinguished research.

The following have been elected **Foreign Members of the Royal Society**: **Academician Professor J. M. Bijvoet**, formerly of the State University of Utrecht; **Dr K. S. Cole**, US National Institutes of Health; **Dr M. Ewing**, Columbia University; **Dr L. F. Leloir**, Institute of Biochemical Research, Buenos Aires.

The **Appleton Prize**, awarded every three years by the Council of the Royal Society in consultation with the Board of Officers of **URSI**, in commemoration of **Sir Edward Appleton**, will be presented this year to **Professor R. A. Helliwell**, Stanford University, for his contributions to the theory of the propagation of electromagnetic radiation in the Earth's magnet-

New Academy Members

ONE of the chief functions of the annual meeting of the National Academy of Sciences is to elect new members. Seventy-five scientists had the honour of membership bestowed upon them recently, bringing the total membership to 950. The new members are:

Bruce Nathan AMES, University of California, Berkeley; **Henry Herman BARSCHALL**, University of Wisconsin; **Alexander Gordon BEARN**, Cornell University Medical College; **Baruj BENACERRAF**, Harvard Medical School; **Kenneth Merle BRINKHOUS**, University of North Carolina; **Roy John BRITTEN**, Carnegie Institution of Washington; **Roger William BROWN**, Harvard University; **Avram Noam CHOMSKY**, Massachusetts Institute of Technology; **James Samuel COLEMAN**, Johns Hopkins University; **Sidney Paul COLOWICK**, Vanderbilt University; **Rodney Lee COOL**, Rockefeller University; **Albert Victor CREWE**, University of Chicago; **Stanley Jerome CRISTOL**, University of Colorado; **Robert Alan DAHL**, Yale University; **William Jefferson DARBY**, Vanderbilt University School of Medicine; **Vincent Paul DOLE**, Rockefeller University; **Richard James DUFFIN**, Carnegie-Mellon University; **Frank Donald DRAKE**, Cornell University; **Pol Edgard DUWEZ**, California Institute of Technology; **Ernest Ludwig ELIEL**, University of Notre Dame; **Hans Peter EUGSTER**, Johns Hopkins University; **Harold J. EVANS**, Oregon State University; **Don Wayne FAWCETT**, Harvard University; **Leon FESTINGER**, New School for Social Research; **George Brooks FIELD**, University of California, Berkeley; **Maxwell FINLAND**, Harvard Medical School; **Joseph Grafton GALL**, Yale University; **John Heysham GIBBON, Jr**, Jefferson Medical College; **Peter Carl GOLDMARK**, CBS Laboratories (recently retired); **Peter Martin GOLDREICH**, California Institute of Technology; **Ralph Edward GOMORY**, IBM; **Erwin Louis HAHN**, University of California, Berkeley; **Jack Rodney HARLAN**, University of Illinois; **Roy HERTZ**, Population Council, New York; **James Gerald HIRSCH**, Rockefeller University; **James Lynn HOARD**, Cornell University; **Roald HOFFMANN**, Cornell University; **George Caspar HOMANS**, Harvard University; **George William HOUSNER**, California Institute of Technology; **Francis Clark HOWELL**, University of California, Berkeley; **Henry Seymour KAPLAN**, Stanford University School of Medicine; **Samuel KARLIN**, Stanford University and Weizmann Institute; **Donald KENNEDY**, Stanford University; **George Brampton KOELLE**, University of Pennsylvania School of Medicine; **Simon Smith KUZNETS**, Harvard University; **Henry Sherwood LAWRENCE**, New York University School of Medicine; **Alexander LEAF**, Harvard Medical School; **Robert Duncan LUCE**, Institute for Advanced Study; **Willem van Rensselaer MALKUS**, Massachusetts Institute of Technology; **Robert Bruce MERRIFIELD**, Rockefeller University; **Kurt Martin MISLOW**, Princeton University; **Oliver Evans NELSON**, University of Wisconsin; **Allen NEWELL**, Carnegie-Mellon University; **Charles Egerton OSGOOD**, University of Illinois; **Robert Lamar PIGFORD**, University of California, Berkeley; **Frederick Chapman ROBBINS**, Case Western Reserve Medical School; **Reed Clark ROLLINS**, Harvard University; **Jerzy Edwin ROSE**, University of Wisconsin; **Saul ROSEMAN**, Johns Hopkins University; **Malvin Avram RUDERMAN**, Columbia University; **Elizabeth Shull RUSSELL**, Jackson Laboratory; **Gertrude SCHARFF-GOLDBERGER**, Brookhaven National Laboratory; **Robert Merton SOLOW**, Massachusetts Institute of Technology; **Alexander SPOEHR**, University of Pittsburgh; **Charles TANFORD**, Duke University Medical School; **Hans Lukas TEUBER**, Massachusetts Institute of Technology; **Lewis THOMAS**, Yale University; **James TOBIN**, Yale University; **Sam Bard TREIMAN**, Princeton University; **Pindaros Roy VAGELOS**, Washington University School of Medicine; **Steven WEINBERG**, Massachusetts Institute of Technology; **John Roy WHINNERY**, University of California, Berkeley; **George William WHITEHEAD**, Massachusetts Institute of Technology; **William Barry WOOD III**, California Institute of Technology; **Robert Walter ZWANZIG**, University of Maryland; the posthumous election of **Dr Solomon Aaron Berson**, Mt Sinai School of Medicine, was also announced by the Academy.

The National Academy of Sciences also announced the election of twelve foreign associates of the academy. Their election brings the total for foreign associates of the academy to 120. The newly elected foreign associates are:

Ronald Percy BELL, University of Stirling, Scotland; **Henri CARTAN**, Institut Henri-Poincaré, Paris; **Alan COTTRELL**, Deputy Chief Scientific Adviser to Her Majesty's Government, Cambridge, England; **Osamu HAYAISHI**, Kyoto University, Japan; **John Cowdery KENDREW**, Medical Research Council Laboratory of Molecular Biology, Cambridge, England; **Ralph von KOENIGSWALD**, Senckenberg Museum, Frankfurt am Main, Germany; **Yuval NE'EMAN**, Tel Aviv University, Tel Aviv, Israel; **Marcel NICOLET**, Institute for Space Aeronomy, Brussels, Belgium; **Rodney R. PORTER**, University of Oxford, Oxford, England; **Ulf Svante VON EULER**, Karolinska Institute, Stockholm, Sweden; **János SZENTAGOTAI**, University of Budapest Medical School, Budapest, Hungary; **Mogens WESTERGAARD**, Carlsberg Foundation, Copenhagen, Denmark.

The members of the National Academy of Sciences also elected **Emanuel R. Piore**, IBM, to a second term of office as NAS treasurer, and the following four persons were elected members of the council of the academy: **Lewis M. Branscombe**, National Bureau of Standards; **Preston Cloud**, University of California, Santa Barbara; **Harry Eagle**, Albert Einstein College of Medicine; and **Frank Westheimer**, Harvard University.

osphere and to the understanding of "whistler" phenomena and very low frequency noise emissions in this region.

Errata

In the book review by N. Sim (*Nature*, 236, 555; 1972) "CBW in Perspective", line 7 should have read "... through this discovery of nerve gases ...".

In the article "Single Hit Aetiology of Human Minor Congenital Malformations unassociated with Major Congenital Malformations" by E. B. Hook and J. J. Petry (*Nature*, 227, 847; 1970), the first sentence of the penultimate paragraph should read: "These conclusions apply only to the malformations under investigation and are limited to infants with up to three minor defects by the number of data available". The third sentence of the same paragraph should read: "Furthermore, particular pairs of minor malformations may show a positive or negative tendency to associate ...".

Reports and Publications

not included in the Monthly Books Supplement

Great Britain and Ireland

The Carnegie United Kingdom Trust. Fifty-eighth Annual Report 1971. Pp. 20. (Dunfermline, Fife: The Carnegie United Kingdom Trust, 1972.) 23
Soil Survey of England and Wales. Soil Survey Records. No. 2: Soils in Herefordshire 1, Sheet SO 53 (Hereford South). By J. M. Hodgson and R. C. Palmer. Pp. vi+81+3 plates. 75p with map; 50p without map. No. 5: Soils in Cheshire 1, Sheet SJ 65 (Crewe West). By R. R. Furness. Pp. viii+99+7 plates. 75p with map; 50p without map. No. 7: Soils in Essex 1, Sheet TQ 59 (Harold Hill). By R. G. Sturdy. Pp. vi+184. £1.25 with 3 maps; 50p without maps. No. 9: Soils in Devon 1, Sheet ST 10 (Honiton). By T. R. Harrod. Pp. vii+150. £1 with 2 maps; 50p without maps. (Harpden, Herts: Soil Survey of England and Wales, 1971.) 23
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